Remarks
ON
DIAPHRAGMATIC HERNIAE.

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IF one may judge from current medical literature, the passage of viscera from the abdomen to the thorax through a defect in the diaphragm is not an uncommon form of hernia. Dr. J. W. Ballantyne found records of 100 cases published between 1888 and 1900; an excellent article on 63 cases was published by Mr. Lawford Knaggs in 1903; in the same year M. Paillard was able to give references to 481 described cases. In spite of the extensive literature, the cause and significance of the defects of the diaphragm which render such hernia possible are ill understood—a natural result of our present imperfect knowledge of the origin and history of that muscle. The history of the diaphragm is linked up with that larger and still unsettled problem—the origin of the mammals, and especially of their peculiar respiratory mechanism. At the present time the diaphragm is regarded by most anatomists as a septum which has grown across the coelomic cavity, dividing it into pleural and peritoneal compartments. When, however, one begins to account for the various characters of the diaphragm, its origin in the neck of the embryo, its nerve supply from the fourth and fifth cervical nerves, and especially for the manner in which this effective part of the mammalian mechanism gradually assumed its present functional character, it becomes evident that its origin as a transcoelomic partition provides a very imperfect working hypothesis. The explanation I offered in the *Journal of Anatomy and Physiology* some five years ago still appears to me the most satisfactory. The pleural cavities are, to my way of thinking, similar in many respects to the tunicae vaginales; both are extrusions or extensions of the primary coelomic cavities, but whereas the testicles have extruded in front of them an extension of all three primary layers from the hind-end of the primary abdominal cavity, the lungs perforate only the inner of the three layers of the fore end of that cavity, and instead of being extruded in the neck, push down this inner layer to form the diaphragm. The lungs as they develop thus become lodged as it were in the body wall, as one might conceive happening in the case of an imperfectly descended testicle at the groin. The inner or transversalis layer at the cervical end of the primitive abdomen was originally composed, as one may see in some modern amphibia, of a dorsal and ventral sheet of musculature, both of which were attached to and acted on the pericardium and roots of the lungs. Hence, with the downward dislocation of the cervical inner sheet to form the diaphragm, the pericardium and heart are also carried within the body cavity. Clinicians are well aware that all nerve manifestations connected with the diaphragm, and with the diaphragmatic aspect of the liver, are still referred to the areas supplied by the original (fourth and fifth) nerve segments.

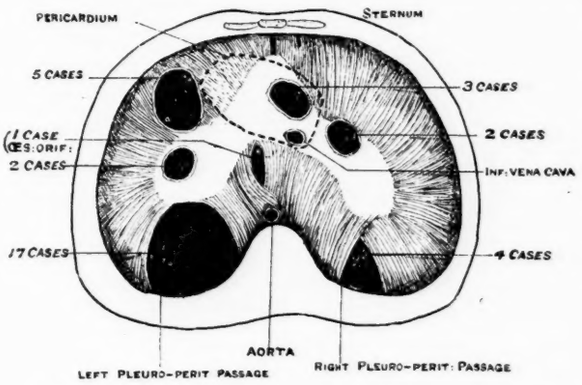

Fig. 1.—Diagram to show the sites of diaphragmatic hernia, as deduced from 34 museum specimens in London. The upper or thoracic aspect of the diaphragm is represented.

I.—*Hernia through the Pleuro-peritoneal Passage.*

My purpose here is to refer to this migration theory of the lungs merely as far as it helps us to understand the various forms of diaphragmatic herniae. In Fig. 1 I have summed up in a diagrammatic manner the results of an examination of thirty-four specimens which show defects of the diaphragm. These specimens are preserved in the medical museums of London. It will be observed that seventeen of these specimens show a defect at the dorsal or spinal part of the left side; four at a corresponding position on the right side. These two defects or openings I have named the pleuro-peritoneal passages, and I regard them as similar to the internal abdominal rings—namely, the points at which the right and left lungs made their escape from the primitive abdominal cavity. The muscle of the crura of the diaphragm which separates these orifices represents the dorsal part of the cervical transversalis, while the sterno-costal fibres of the diaphragm represent a ventral part of the same sheet. In the normal course of development these orifices are closed before the end of the second month; but one can understand, as is the case with the internal opening of the tunica vaginalis, that the process of closure may be delayed or fail. The cause of the failure we do not know, but one may safely presume that it is of the same nature as the cause which arrests the union of the primary elements of the upper lip and leads to the condition of harelip. It is not the respiratory movements at birth which draw the abdominal contents into the thorax; the atelectatic and compressed condition of the left lung (when hernia is situated on the left side) shows that the abdominal contents had entered the left thorax long before birth, probably owing to the spasmodic movements which are known to occur in the fetus *in utero*. The size of the aperture in those children which died at birth is larger on the left side than on the right; on the left side the deficiency varies from one to two thirds of the floor of the left pleural cavity; on the right side the size is rarely so much as a third of the right pleural floor. The explanation of the larger orifice on the left, and of its greater frequency (17 on the left to 4 on the right in the present series) is the same as leads to the greater frequency in the displacement of the right kidney in the adult. The contents of the left hypochondrium are much more movable and plastic than those of the right; hence, when the contents of the abdomen are compressed in the fetus with an open pleuro-peritoneal passage, the massive liver blocks the orifice on the right, while the left contents may easily slip through. The case is otherwise when dislocation of the kidneys in the adult is considered. The mobility of the contents of the left hypochondrium safeguards the left kidney; the relative immobility of the liver renders the right kidney subject to all compressing influences which act on the right hypochondrium.

Of the 4 cases of hernia through the right pleuro-peritoneal passage, 2 died at birth, 1 lived one and a half hours, another 4 hours. Of the 17 cases on the left side, 12 died at birth or were stillborn, 1 lived six hours, 1 six weeks, 1 three months, 1 to the age of 34, and 1 to old age. Dr. Penrose Williams has recorded a case in a man of 77, and a case of Dr. Edmund Cautley, recorded by Mr. H. D. Williams, lived to 9 months. Dr. R. J. Gladstone has described the case of an anencephalic fetus where the fused kidneys had entered the left side of the thorax by the pleuro-peritoneal passage; and also the case of a woman of 96 where the right passage was distended by a fatty hernia. Professor A. M. Paterson of Liverpool and Professor Arthur Robinson have dealt with the anatomy of this form of diaphragmatic hernia. Mr. Lawford Knaggs, when he reported a series of 63 cases seven years ago, said he was aware of only two cases in which an operation had been attempted—a case of his own and one recorded by Mr. James Berry. Neither of these cases was congenital hernia of the pleuro-peritoneal passage. From an examination of the present series of cases I am of opinion that a considerable number of these cases might be successfully operated on if the condition were recognized at the time of birth.

II.—*Hernia through the Oesophageal Orifice of the Diaphragm.*

As may be seen from Fig. 1, there was only one specimen in the series of 34 which showed a hernia at the oesophageal orifice. This specimen is in the museum of Guy's Hospital, and shows the greater part of the stomach in the right pleural cavity. Mr. Lawford Knaggs has described several cases; Mr. W. A. E. Waller has recently placed a remarkable one on record in a young man of 19; Dr. Barclay-Smith found the condition associated with great dilatation of the sigmoid colon. Mr. Rigby and the writer have recorded a case in which a partial hernia of the stomach through the oesophageal orifice was associated with enteroptosis.

III.—*Herniae through the Right Leaflet of the Diaphragm.*

As shown in Fig. 1, there were only two cases of hernia through the right leaflet—one in a child of 4 months, where a process of the liver, about the size of a cricket ball, occupied a sac formed from the expanded central tendon; and another, also in a child, where a process of the liver lay free in the floor of the right pleural cavity. There can be little doubt that both of these were congenital in origin and were produced by an abnormal development of the liver within the septum transversum of the embryo. Professor Arthur Thomson has recorded a similar occurrence in the rabbit, the abnormal process of liver resembling an accessory heart.

IV.—*Hernia into the Pericardium.*

There were three cases of hernia into the pericardium through a perforation of the central tendon of the diaphragm. In the case of a man of 50 a process of omentum occupied the orifice; in a man of 47 a process of liver formed the extruded part. In neither of these cases was there a hernial sac, but in the third case, in a child at birth, the process of liver was enclosed in a sac formed by the central tendon. It is possible that the two cases in adults were the result of a former injury. Elder and Postlethwaite report a case where the lobe of a malformed liver occupied a diverticulum of the right central tendon, the diverticulum reaching the level of the second rib.

V.—*Hernia through the Left Dome of the Diaphragm.*

Herniae through the left pleuro-peritoneal passage are four times more common than through the right, and the same statement holds true as regards the various forms of hernia which occur at the domes and sterno-costal parts of the diaphragm. As may be seen from Fig. 1, herniae occur at two sites on the left dome—an anterior or sterno-costal site, and a posterior or central site. In the present series of 34 cases there were 5 at the sterno-costal site and 2 at the central. Of the sterno-costal form all were in adults varying in age from 19 years to 50, and in every case there was a history of a former injury by which the abdomen had been suddenly and forcibly compressed. There was no specimen of hernia at this site in a newly-born child. Cases of hernia in adults as the result of a former injury have been recorded by Mr. Lawford Knaggs, Professor A. M. Paterson, Mr. J. P. Roughton, and Mr. Don. In the two central cases the abdominal contents were enclosed by a sac formed from the extended diaphragm in one case (a child which lived forty-three hours), and in the other (a child at birth) the orifice in the diaphragm was free, and there was no sac. Both of these cases were clearly of congenital origin.

SUMMARY.

Herniae through the diaphragm fall into two groups, the congenital and the acquired, the former being to the latter in the present series of 34 cases as 26 : 8. The congenital herniae are chiefly those which occur at the unclosed pleuro-peritoneal passages (21 cases), the others being formed (5 cases) by developmental extrusions of the abdominal viscera, chiefly liver, through the septum transversum. In a certain proportion of congenital cases it appears possible to adopt surgical measures for the cure of the condition.

I should like to avail myself of this opportunity of expressing my indebtedness to the curators of the various hospital museums in London for the facilities given me of examining the valuable specimens under their care.

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TYPHOID PERFORATIONS AND PERFORATIONS OF THE GALL BLADDER.

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PERFORATION of the intestine in typhoid fever demands our most careful study because of its frequency and danger. Physicians and surgeons are together bound to do their utmost to lower the high mortality-rate that obtains at present in this the most lethal of all the complications of typhoid.

Osler says that 1 out of every 3 deaths occurring during typhoid fever is due to perforation, and this estimate of its frequency is confirmed by the studies of the late J. Allan Scott. If the same percentage of deaths occurs in Canada as are estimated by Brown and Taylor to obtain in the United States, we have 1,875 deaths each year in our Dominion from typhoid perforation.

The locus is generally in the terminal two feet of the ileum, but, of course, it may occur at any part of the ileum, jejunum, large intestine, or appendix. The pathogenesis of typhoid perforation is well known. The coagulation necrosis of the lymphoid tissue opposite the mesenteric attachment, where the blood supply is poor, has been clearly described by many writers.

The problem before our profession is to lower the mortality-rate; therefore, the first question is, What are the causes of the high death-rate in typhoid perforation?

1. The condition of the patient at the time of its occurrence. Towards the removal of this cause we can do little, although it adds greatly to the difficulties of recognition.

2. The difficulties in diagnosis. This is almost the whole question at the present time. Once perforation has occurred, we may agree pretty well on the remedy to be applied and on most of the details of carrying that remedy into effect; but how to make the diagnosis—that is the question above all others in typhoid perforation.

To begin with, let us clear the ground of one or two erroneous ideas. We must not expect perforation of the intestine in typhoid to be announced by definite and distinct symptoms. So long as we look for the same abrupt onset that takes place when a stomach or appendix ruptures in a patient who is in full health at the time, and for symptoms that are unmistakable, so long we may go on as we have been doing, and make our diagnosis when it is too late to be of benefit to the patient.

My second point is that we must clearly appreciate the difference between perforation and peritonitis. The time has passed when we could say that perforation could only be recognized by the resulting peritonitis. A third-year student can diagnose peritonitis; the master should be able to diagnose perforation.

It is generally a good rule to doubt a diagnosis when the symptoms lack in harmony, but in typhoid perforation the absence of harmony is a diagnostic point in favour of perforation. The change for the worse in the patient is often out of all proportion to the abdominal symptoms. The first symptom is pain, and, as a rule, it comes on suddenly. Abdominal pain is the danger signal, and should always receive immediate attention. In rare cases the pain comes

on gradually; this is the exception, however. It is commonly referred to the lower abdomen, often to the right side, but may be referred to the umbilical or epigastric regions. Tenderness is usually present, and at first is localized. The initial pain and localized tenderness are the most important indications of perforation, and should be considered and differentiated from the more widely diffused pain and more generalized tenderness that appear later, and indicate the presence of peritonitis.

Rigidity and muscle spasm are now to be looked for. Greater resistance of one rectus muscle is significant. The difficulty is that typhoid patients often complain of the abdominal pain, and are generally more or less tender in the right iliac region. A differential point is that the pain and tenderness of perforation are associated with an altered condition of the patient. The nurse or the friends generally notice that something has occurred. The expression is different. There may be a clammy perspiration. If the pain, tenderness, and rigidity are accompanied by nausea or vomiting, as is the case in a considerable percentage of cases, their importance as a diagnostic sign is greatly enhanced.

One should not conclude that there is no tenderness present without a rectal examination. In some cases the perforated loop lies low down in the pelvis. The patient may then not complain much of pressure upon the abdomen, but forward pressure of the finger in the rectum will at once cause him to cry out. It is in these cases that complaint may be made of frequent and painful micturition.

Later on, when the infection of the peritoneum has become diffused, and peritonitis has developed, careful inspection may note lessened abdominal movement during respiration, particularly of the lower abdomen and in men. The absence of liver dullness is only of value when the abdomen is quite flat, and especially if its presence has been previously noted. Alteration in the pulse, temperature, and respiration may be confirmatory, but it is often remarkable how little they are disturbed. The blood pressure is sometimes increased. I have not found the presence or absence of leucocytosis a symptom upon which much reliance can be placed. It may be present without perforation and absent when perforation is present.

None of these symptoms are new. They are mentioned in all textbooks. The point is to be on the alert, and not to allow them to pass unnoticed.

There were treated in the Montreal General Hospital from 1897 to May, 1910, 2,051 cases of typhoid fever. During this period there occurred 93 cases of perforation. In this series, then, perforation occurred in 4.53 per cent. of the cases. In addition, I operated upon three cases of typhoid perforation in another hospital and upon one case in a private house; of these four, three recovered. We have, then, 97 perforations, of which 78 were operated upon, and 24, or 30.76 per cent., recovered.

If we now examine the statistics for the various years, we find that the percentage of recoveries has steadily increased. During the year 1909 there were 16 perforations; 9 were operated upon, and 9, or 56.25 per cent., recovered. Our figures for the year 1909 and four months of 1910, or up to May last, are the best of all. During this period there were 22 perforations, 19 of which were operated upon, with 9, or 47.36 per cent., recoveries. In this series of 22 perforations there were two very promising cases lost, apparently, by delay in obtaining consent after the perforation was diagnosed.

Annual deaths in Canada from typhoid fever ...	1,875
Montreal General Hospital, 1897 to May, 1910:	
Cases of typhoid	2,051
Cases of typhoid with perforation... ...	93 (4.5%)
Cases of perforation in Montreal General Hospital	93
Author's cases outside hospital	4
Total cases of perforation	97
Cases submitted to operation	78
Cases not operated on	19
Recoveries after operation	24
Percentage recovery (24 out of 78)	30.7

Recent Results.

Perforations, January 1st, 1909, to May, 1910 ...	22
Submitted to operation	19
Recoveries	9
Percentage recovery (9 out of 19)	47.3

Type of Fever in which Perforation Occurred.

Severe	50
Moderately severe	20
Mild	1
Ambulatory	6
Type not described	16
	93
Associated with severe haemorrhage	26
Multiple perforation—6 cases ... (per cent.)	7.7
Secondary operation for secondary perforation in 4 cases: Recovery	1

The increased percentage of recoveries is, no doubt, in part due to the character of the epidemic, but chiefly to earlier diagnosis and earlier closure of the opening. The earlier diagnosis has been due largely to the development of the idea among the resident hospital staff that it is a serious reflection on their skill and professional attainments to fail to recognize a typhoid perforation, no matter how slow and chronic may be its appearance. The final step towards lessening the mortality-rate in typhoid perforation is the recognition by every one of us that it is a serious and humiliating oversight to fail to recognize a perforation when it occurs.

The difficulties are greatly increased by the dull and apathetic state so common in typhoid, but these difficulties should only stimulate us to greater watchfulness.

I think it may be said that the diagnosis is, in many cases, to a certain degree uncertain. For this reason, in all cases in which we think a perforation is present, but where there is a reasonable doubt, an exploratory incision is to be recommended. I have twice opened the abdomen without finding any perforation. So far as I could observe, no harm resulted. This exploratory incision, undertaken after due and careful consideration, may sometimes save a life.

While it is quite true that the earlier the perforation is closed the more limited is the infection and the better the prognosis, yet some of our cases have been operated upon and have recovered when the condition, because of delay and advanced peritonitis, has seemed almost hopeless. Our experience in these cases would indicate that we are not justified in withholding the only chance there is unless they are really *in extremis*.

It may be said, I think, that if perforation occurs into the free peritoneal cavity the patient cannot recover unless the opening is closed. The only perforations that do not prove fatal unless closed by the surgeon are those occurring in the large bowel and between the layers of the mesentery. It has been demonstrated at the operating table that a small perforation in the small intestine may be temporarily closed by omentum, but that such cases would have recovered if they had been left alone is open to very serious doubt.

I have opened one abscess in the loin—post-typhoid—that seemed clearly to have been secondary to typhoid perforation of the colon.

If now we analyse our cases we find that perforation is more common in the severe forms of typhoid. Fifty of the perforations have occurred in cases described as severe, twenty in moderately severe cases, and one in a mild case. Six perforations occurred in ambulatory cases, and in one of these it seemed highly probable that perforation had occurred sixty hours before admission to the hospital. Strange to say, this man recovered.

Of the 97 cases in which perforation occurred, only 78 were submitted to operation. Nineteen cases were not operated on for various reasons, the chief being failure to obtain consent from the patient, parents or guardians, the desperate condition of the patient, and non-recognition of the lesion.

In several instances the patient's chances have been lessened by the delay in getting consent. One conspicuous example was a man, a patient in the Typhoid Emergency Hospital. The perforation was promptly recognized, and the patient sent to the Montreal General Hospital. He then refused operation, and insisted upon being removed to his home in a distant part of the city. At his home better counsels prevailed, and he was brought back to the hospital. Two long rides in the ambulance and a delay of twelve hours changed an early promising case into an all but hopeless one. These delays from failure to obtain consent are of too frequent occurrence, one reason probably being that we do not promise enough

and one of my objects in this paper is to show that our extreme pessimism is wrong, unscientific, and not sustained by our later experience. Surely, if 50 per cent. of these desperate and otherwise hopeless cases can be saved, our pessimism should give way to a good, vigorous optimism.

Twenty-six of our cases of perforation were associated with haemorrhage. Among these are not included instances of small blood-stained stools but only those cases in which the quantity of blood lost amounted to several ounces. As a class they are unfavourable. Three of them, however, recovered.

In six cases, or 7.7 per cent., more than one perforation was found at the time of operation. The perforations were generally not far removed from each other.

In four cases a second operation was undertaken for second perforation. One of these recovered. In two, recovery would have followed the second operation had not a third perforation occurred—in one case seven days after the second operation and in the other twenty-one days after the second operation. In one case two operations were performed. At the first operation one perforation was closed. At the second operation two perforations were closed. The child finally died, and at the autopsy the lower two feet of the ileum were necrotic, twelve perforations being counted.

Haemorrhage and perforation occurring subsequent to the operation for closure must be placed among the causes contributing to the high death-rate. Can they be prevented?

This raises one of the questions of technique. Is it enough to simply close the perforation found? What other procedure may be adopted?

It has been my practice to close the opening in the simplest way, that is, by one or two sutures through all coats, and a row of Lembert sutures placed transversely at right angles to the long axis of the bowel. I then infold in a similar manner with a row of Lembert sutures all the ulcers found to have a thin, suspicious, necrotic-looking base. Two alternatives suggest themselves, and have been carried out in a few of our cases. In two the suspicious portion of gut was resected with end-to-end anastomosis. They both died. In two the suspicious-looking gut was delivered, and cared for outside. The gut was wrapped in gauze, and allowed to rest on the abdominal wall. These two, after doing well for a time, eventually succumbed, one of them from pneumonia.

Resection should probably be reserved for special cases in which the danger of further perforations over a considerable section of intestines seems imminent, and the patient's general condition warrants undertaking an operation that requires a little more time and a little more shock. I have not seen a report of any series of cases treated by resection, but I am under the impression, from conversations held with surgeons, that resections in typhoid perforations have not been followed by any improvement in the mortality-rate. I believe, however, that it should be carried out under favourable circumstances and in selected cases.

The variety of anaesthesia to be employed is a question of more than passing interest.

We will all, I think, agree that all preparations, including the wash-up, should be done before any general anaesthesia is used.

Local anaesthesia possesses many advantages, and should be used whenever possible. In the case of foreigners who cannot understand our language, and in children, it is difficult, but it undoubtedly should be the method of choice. I must say, however, that most of my cases have been operated upon under general anaesthesia, and I cannot say that I have seen much harm from it. On the contrary, these cases are almost all surprisingly well during the ensuing twenty-four or forty-eight hours. If the patients are vomiting, this is a very forcible reason for using local anaesthesia, as we thus lessen the danger of an aspiration pneumonia.

There is one point to be remembered when operating for typhoid perforation under local anaesthesia, and that is, that when the abdomen is opened there may not at first seem to be any indication that a perforation has occurred. No gas or fluids may escape, and, so far as can be seen through a small incision, the peritoneum and intestine may present a nearly normal appearance. This occurred to me on one occasion. The symptoms in this case had been unusually well defined, and that there was a perforation seemed almost beyond question. On pulling up the lower end of the ileum the patient complained so loudly that I gave a little ether. Then, when the ileum was finally delivered, a small perforation was found nicely closed by adherent omentum. If this had been overlooked, doubtless a leakage and peritonitis would have followed soon afterwards.

The subject is one of great interest. The death-rate must necessarily remain high, but watchfulness, exploratory incisions in doubtful cases, and prompt closure should save a goodly number of typhoid perforations.

I take this opportunity to report a case of typhoid perforation of the gall bladder.

The patient was a little girl 10 years of age, referred to me by Dr. Molson. The typhoid had been of moderate severity. At the end of the third week, after three days of normal temperature, she complained of pain in the right hypochondrium. The following day an ovoid swelling appeared in the situation of the gall bladder. It felt as large as a turkey's egg, was exquisitely tender, and moved up and down during respiration. It was accompanied by a rise of temperature and acceleration of pulse. Under local applications the tumour became less tender and perhaps a little smaller. There was no jaundice. On the fourth day of the tumour another accession of pain decided me to operate. I opened directly into an abscess cavity, and in the abscess lay the gall bladder perforated in three places. The walls were everywhere necrotic. There were no stones. I did a cholecystectomy, and the little girl made a smooth and uneventful recovery.

The following is the report of Dr. Wolbach, pathologist in chief to the hospital:

The specimen is slightly enlarged, thin-walled gall bladder 10 by 4.5 cm. in its greatest diameters. The outer surface is much congested and presents numerous yeliowish raised areas, in a few of which there are perforations extending through the wall. The mucosa is dark red, and contains, in addition to the perforations, many ulcers, 0.1 to 0.5 cm. in diameter. The large ulcers have yellow necrotic bases with undetermined edges. The thickened floor and the surrounding puriform infiltration of these ulcers produce the swelling noted on the external surface of the gall bladder.

Incision through the serous coat into the wall of the gall bladder in the neighbourhood of the perforations and ulcerations shows extensive oedema and puriform infiltration into the surrounding tissues.

Kaiserling specimen preserved.

Microscopic Examination.

The epithelium of the mucous membrane is nearly wholly absent; a few islands of epithelium represent deeper parts of folds in the mucosa. The line of the basement membrane is everywhere intact, except over the ulcers; so that the general loss of epithelium is probably post-operative. Throughout the sections examined there are evidences of oedema and many small areas of haemorrhage.

In thickened parts of the wall there is marked infiltration with lymphoid and plasma cells, and large mononuclear cells, many of which are finely vacuolated (compound granular cells).

Young connective tissue cells are present throughout the sections where there is oedema. The large mononuclear cells (endothelial cells) are occasionally grouped in large numbers about vessels, in lymphatics, and in areas of haemorrhage.

The above changes are most marked in the outer half of the gall-bladder wall—that is, subserous tissues and the elevated areas noted in the gross specimen are due to necrotic areas in this region, surrounded by granulation tissue. Near one large necrotic area there is a large vein with an early organized and canalized thrombus.

In the areas of surrounding necrotic tissue there are many polymorphonuclear leucocytes, but even here the number is not greatly in excess of the large mononuclear cells.

A few short bacilli are present in necrotic areas (methylene blue stain). No other bacteria can be found.

The blood vessels everywhere are engorged, but there is no evidence of recent thrombus formation.

SUMMARY.

Necrosis of gall-bladder wall following marked infiltration of the outer half with lymphoid, plasma, and endothelial cells, in which respect it differs from an ordinary suppurative process. The extensive oedema is possibly due to thrombosis of vessels, though only a single organized thrombus could be found. The character of the cellular reaction supports the theory of a direct infection of the gall-bladder wall by the typhoid bacillus as against pyogenic infection, or infection secondary to a cholecystitis.

The only demonstrable bacteria in the lesions are bacilli consistent in morphology with the typhoid bacillus.

The route of infection could be either through the blood stream or through lymphatics from infected lymph nodes which may be incorporated in the wall of the gall bladder.

BACILLUS COLI INFECTIONS, WITH SPECIAL REFERENCE TO THEIR RECOGNITION AND COMPARATIVE FREQUENCY.

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During my first fourteen years in practice I did not meet with, or at all events recognize, any case of infection by *B. coli*. This is not surprising in view of the inadequate treatment which the subject receives in the average text-book. I am really indebted to my friend Dr. Houston, of Queen's University, for directing my attention to the subject some three or four years ago. As my observations have been restricted to cases occurring in an ordinary general practice, my experience may afford some index of the frequency of these infections, and possibly also some indication as to the numbers which are either overlooked or misunderstood. That they are frequently overlooked at present is, I think, certain; and until more accurate clinical pictures of various types are forthcoming I fancy that it will be so in the future.

I have found that the number of these cases has increased in my practice as I adopted the culture method of investigation with greater uniformity. This method is, in my experience, not inconvenient, and the increased interest which it arouses more than outweighs the extra trouble involved. It will be observed that an accurate clinical diagnosis of some (if not all) of the cases referred to below would be impossible, and that Case IV illustrates the striking benefit which may be anticipated from appropriate vaccine treatment, and thus accentuates the practical importance of accurate diagnosis in this type of infection. I propose at present to deal with some of the cases I have met with in the last *six months*.

Case I.

A child of 10 months, scalded on buttocks and legs, developed diarrhoea on the third day; next day convulsions, with temperature 104°; died on sixth day, apparently from broncho-pneumonia and cardiac failure. Some hours prior to death large wheals appeared all over the body with petechiae. One of the wheals (on the front of the chest) was painted with lin. iodi, about two hours prior to the fatal issue, in order to sterilize the skin; and a hypodermic syringe, previously boiled, was used to obtain a specimen of the exudate. An agar tube was inoculated with a loopful of the contents of the syringe; it proved to be a pure culture of *B. coli*. At the same time one of the blisters on the leg was snipped with sterile scissors, and the platinum loop (previously flamed) was passed into the blister, and then used to inoculate a second agar tube. This also yielded a pure culture of *B. coli*.

Cover-glass preparations from the contents of the blister and the syringe showed a Gram-negative bacillus; no staphylococci or streptococci were seen. There is, therefore, strong presumptive evidence that this was a pure *coli* infection, its source being the child's own motions, and the route of infection being the extensive raw surface on the buttocks and legs.

With regard to the pathogenicity of *B. coli*, one need only note in this connexion that it has been shown by several observers that the virulence of these organisms is increased in typhoid fever and catarrhal conditions of the intestine. There are, therefore, theoretical grounds, as well as the specific evidence afforded by this case, for the statement that in cases where diarrhoea is associated with scalds, especially of the buttocks and lower extremities in young children, there is a grave risk of a fatal septicaemia from *B. coli*, a fact which, I confess, I had not properly appreciated.

The next case appears to raise some points of interest in connexion with "anomalous" febrile attacks and aberrant types of pneumonia.

Case II.

Man, aged 56; sore throat and fever for a week; rigor tenth day; consolidation of left lung; abundant prune-juice sputum; temperature 103–104° for next four days; then attack aborted. Seven days later became delirious, and temperature reached 105°, but again touched normal in three days. There were occasional subsequent rises to 100°, but he gradually improved and eventually recovered. He was in bed about seven weeks.

A cover-glass preparation from the sputum in the first twenty-four hours of the pneumonia showed a diplococcus (without obvious capsule) and a bacillus in large numbers. A culture was grown from the sputum in the hope of isolating the pneumococcus; the bacillus grew luxuriantly and the attempt proved futile. It was not until the patient had been convalescent for a month that I accidentally discovered that this culture had a slight odour and contained a motile bacillus. I at once obtained a sample of the patient's urine. It also contained a motile organism. I made a culture from the urine and submitted it with a subculture from the sputum and some of the patient's serum to Dr. Sidebottom, of Victoria University, who very kindly agreed to investigate the matter for me, and to whom I take this opportunity of expressing my thanks. His report is as follows:

"The two cultures were plated and found to be pure. They consisted of a small bacillus, motile and Gram-negative. Both organisms fermented glucose and lactose with production of gas. They produced clotting and acidity in litmus milk. The production of indol was not clear. Both organisms appeared to belong to the *B. coli* group; but there were small differences between them, the organism isolated from the urine showing a tendency to form long filaments. There was no indication of agglutination or stoppage of motility when the organisms were brought into contact with the patient's blood (10 : 1)."

With regard to the agglutination reaction, it is well to recollect that it was about three months from the onset of the illness until the agglutinating capacity of the serum was tested. Dudgeon and Sargent[1] failed in 52 out of 68 cases of peritonitis due to *coli* organisms to obtain any reaction with a dilution of 1 in 10. Wilson's results[2] were similar in the case of coliform organisms isolated from the urine. A negative reaction in this case, therefore, is not evidence of any great weight against the conclusion that the bacilli isolated from the sputum and urine had a specific connexion with the patient's illness. A trace of albumen was found in the ordinary routine examination of the urine in the acute stage of the pneumonia, but I wish to state emphatically that there were no bladder symptoms all through. I suggest, therefore, that the case was one of mixed infection by the *B. coli* and *pneumococcus*, that the primary focus of infection was the throat or lung, and that the infection of the urinary tract was secondary when comparative immunity had been achieved, and consequently no obvious inflammation of the urinary tract resulted.

My experience points to the conclusion that in cases of primary infection of the urinary tract there are some signs of cystitis, which may be so slight as to be easily overlooked altogether, or so severe as to cause grave anxiety about the patient's life. But whether the infection of the urinary tract be primary or secondary, mild or severe, the bacilluria appears to persist in most cases for weeks or months. I have recently seen a patient suffering from persistent recurring headaches who has a history of typhoid fever sixteen months ago. He has had no bladder symptoms since, but he has a *coli* bacilluria. I have not been able to find typhoid bacilli, though, of course, it was with the idea of possibly finding typhoid bacilli that I made cultures from his urine.

The following is a good example, I think, of the mildest type of infection of the urinary tract:

Case III.

A primipara, aged 32, had a normal labour; she was unable to pass urine, however, during the first week. A silver catheter, boiled immediately before use, was passed night and morning. Temperature and pulse were normal until the fourteenth day, when she had a scarlatiniform rash interspersed with urticaria-like wheals which "burned" slightly but did not itch; her temperature was 99°, and pulse 86. Careful inquiry from the patient and the nurse elicited the fact that there had been slight dysuria on the first three or four occasions on which she urinated naturally. She had also appeared a little feverish on the evenings of the twelfth and thirteenth days, but she had not felt ill, and my attention was not called to the matter until the rash appeared. A specimen of the urine (obtained by catheter) was found to be acid; it contained a little pus and a motile Gram-negative bacillus, which on culture was found to be one of the colon group. The patient's blood in dilution of 1 in 10 agglutinated the bacillus, but did not completely inhibit motility; it was found, however, that the observer's blood had an almost equal agglutinating effect upon this bacillus. Beyond rest in bed for another week no treatment was adopted, and no further symptoms developed. The bacilluria persisted for five weeks afterwards, during which the patient appeared in perfect health. No further observations were made.

With regard to the rash, clinically it appeared to be related to the slight febrile condition. If it were, the case suggests that some of the erythemata of the puerperium may be due to a mild *coli* infection which escapes notice. Of course it may be urged that erythemata are uncommon in this kind of infection, but an obvious reply is that the great majority of people do not develop urticaria after eating shell-fish, whereas a minority do. Similarly bacterial toxins may in patients of a certain idiosyncrasy

produce puzzling erythematous eruptions, but fail to do so in the majority of patients suffering from the same form of toxaemia.

The next case illustrates the severe type of infection of the urinary tract. It represents a class of case which might easily be, and I believe has been, mistaken for appendicitis, and be submitted to a useless operation.

CASE IV.

Woman, aged 28; pregnant five and a half months; had vomiting and rigors. Temperature 103°, pulse 130, respirations 32, abdominal tenderness, especially over the right iliac region and towards the right kidney; some dysuria. During the first week, sleeplessness, persistent vomiting, abdominal pain and dysuria were the most prominent features. The urine was acid, contained some pus, which increased in quantity later, and a motile bacillus in enormous numbers, which was Gram-negative, and on subsequent culture proved to be one of the colon group—that is to say, it produced acidity and dotting of litmus milk; fermented lactose; gave a luxuriant brown growth on potato, and did not liquefy gelatine. A three days' broth culture gave a deep purple colour when 1 c.cm. of a 0.02 per cent. solution sodium nitrite and 1 c.cm hydrochloric acid were added.

In this case alkalis and morphine (by the mouth) proved valueless, owing to the persistent vomiting, and at the end of the first week the case was looking very grave. It was then decided to try the effect of a vaccine made from a six-hours' subculture of the patient's own bacillus. A dose of fifty millions (approximately) was injected on the morning of the seventh day of the illness.

The patient slept well that night without a narcotic. Next morning she said she felt "perfectly well." Her temperature, etc., were normal; the vomiting, the dysuria, and abdominal pain had disappeared. On getting up, however, a week later, there was some return of these symptoms, and, after repeated entreaties on the part of the patient, another dose of vaccine was given, and was followed by the same rapid and gratifying improvement as before. Her subsequent convalescence was satisfactory, and her accouchement was normal.

Before giving the first vaccine injection, the agglutination reaction of the patient's serum was tested against the bacillus grown from the urine. It was found that, whereas observer's serum in a dilution of 1 in 10 had no appreciable effect on this bacillus at the end of one hour, the patient's serum agglutinated it instantaneously—that is to say, it was found to be agglutinated as soon as the preparation was focussed. A considerable number of similar observations with different dilutions were subsequently made in this case. The general result may be stated thus: Obvious clumping occurred in a dilution of 1 in 50 in a few minutes; but even with a dilution of 1 in 10 motility was not completely inhibited at the end of two hours. Careful comparisons with a normal serum or serums are, I think, imperative if any definite conclusions are to be drawn in *coli* infections from the agglutinating capacity of the patient's serum.

CONCLUSIONS.

I do not propose to elaborate further any of the points raised in connexion with these cases at present; but I may briefly summarize the conclusions at which I have arrived from personal observations:

1. These infections are commoner and more important than the average textbook would lead one to believe.
2. They vary within wide limits in respect of their virulence, and therefore in respect of their clinical features.
3. Their recognition is important, not merely from the standpoint of accurate diagnosis, but also from the standpoint of efficient treatment in the severer types.
4. The slighter cases are apt to be overlooked altogether, the severer cases are apt to be misunderstood.
5. Clinical observations alone, however conscientious, are at present quite inadequate; bacteriological evidence is a *sine quâ non* of decisive diagnosis.

REFERENCES.

[1] *The Bacteriology of Peritonitis*, p. 147. [2] *Journal of Hygiene*, vol. viii, September, 1908.

WE have received from Continental Travel, Limited, a handbook giving an account of Swiss resorts open in the winter, together with information as to the facilities they offer for winter sports and as to the accommodation offered at hotels. Also dealt with are the cruises to the Mediterranean which the company organizes during the spring. The book is of value since, apart from other points, exact information as to Swiss winter resorts is not elsewhere easy to obtain. It is edited by Sir Henry Lunn, and includes a number of articles on winter experiences by different writers, and also a record of a tour round the world. Another similar tour is now it seems in preparation, the leader being, we note, Dr. Thwaites, a former army medical officer.

ON THE DIET OF A PATIENT BEFORE OPERATIONS UNDER GENERAL ANAESTHESIA.

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I DO not propose to deal here with special cases, each of which must be treated on its own merits; however, as ordinary healthy patients who have to undergo some simple operation are so frequently subjected to an absurd period of starvation, or are given such totally unsuitable food before an operation, I think it is worth while devoting a little consideration to the subject.

I am fully aware that many members of the profession, especially those of the older school, will disagree with my views; and surgeons, anaesthetists, and general practitioners will be found to have opinions differing widely, not only from mine, but also from those of each other on this important question.

It is a peculiar fact that, though the diet of a patient before an operation is regulated mainly on account of the anaesthetic, it is quite exceptional for the anaesthetist to be consulted on the matter at all. In spite of this, however, he and his much maligned drugs are given all the blame for any unpleasant symptoms that may arise during or after the operation. Of course, a careful and experienced anaesthetist will obtain far better results under any system than one who is less competent; but his results might be enormously improved by subjecting the patient to a more reasonable and thoughtful preparation.

In an able article in the *Lancet* of April 4th, 1908, p. 993, Dr. William Hunter pointed out that the so-called "delayed chloroform poisoning" was actually due to starvation before and immediately after operations; and as this opinion emanated from a credited physician and pathologist, and not from an anaesthetist, I presume it will be accepted as being at least unbiassed. In the general conclusions at the end of his paper, he lays down that post-anaesthetic vomiting is essentially toxaemic, due to the profound depression of the liver function, which will be greater if a liver, already weakened by disease or by poor nutrition, be further unduly weakened by food having been withheld for many hours before the administration. This enforced abstention from food before the administration of an anaesthetic is, he says, largely responsible for the fatal effects of "delayed chloroform poisoning," and such effects could probably be completely prevented by taking care that the patient always had a very nutritious, well-sweetened, and easily digestible meal, two or three hours before the operation.

Moreover, the Sisters in the wards of the hospitals inform me that a result of the preparation of all ordinary operation cases by a routine method is that the patients whose turn comes at the end of a long list in the afternoon are always much more sick, and have far more straining than those who are done earlier, and have therefore not been over-starved to the same extent. It is the practice in many hospitals to prepare every patient as though he would be operated upon at 2 o'clock; whereas, if there be five or six cases to be done one after the other, the unfortunate victim who comes last on the list will be starved for some four or five hours longer than is actually intended. The old idea was that he would therefore have an absolutely empty stomach, and so would probably not vomit at all. Experience, however, shows that instead of suffering less after the operation, he really has far greater discomfort than the patient who is done first, and who is thereby saved from the miseries of the prolonged starvation.

At nursing homes, also, I am told that the patients nearly always suffer more after an anaesthetic if they are prohibited from taking anything by the mouth on the morning of the operation when the latter takes place at 10 or 11 a.m.

The usual errors in preparing patients for an anaesthetic, however, are by no means confined to the length of time during which they are made to abstain from food. They are frequently also given an unsuitable *kind* of food for their last meal. Thus, one sometimes hears of healthy patients being made to swallow some concentrated meat essence at 6 o'clock in the morning. This might be

possible at lunch time, but the stomach is not accustomed to this kind of food at the earlier hour, and it will certainly resent it on a day of such anxiety.

Besides, why should it be concentrated? A little extra water in the stomach can certainly do no harm. As every medical man knows, if a patient be starved all night, and given nothing whatever by the mouth in the morning, he can still bring up fluid which would be at least as likely to choke him as the extra water would; while, on the other hand, the amount of straining required to bring up this thick viscid mucus is greatly in excess of that required to get rid of the more liquid substance, and the patient's discomfort is increased accordingly.

Contrary to the usual belief, ether cases generally suffer less discomfort after regaining consciousness than chloroform cases; and this, I think, is partly explained by the fact that the secretion in the stomach is more copious and liquid when ether is employed. So I have gradually been converted to the opinion that the want of food, and particularly of fluids, is an important factor in causing the disagreeable after-effects which are usually falsely ascribed solely to the anaesthetic.

It should further be remembered that the stomach develops a habit of expecting, and demanding in no uncertain manner, a meal at regular definite times; and that, if it be disappointed in this expectation, it will cause its owner to suffer if he takes an anaesthetic, goes on board ship, or often even if he merely happens to encounter some unpleasant smell. Hence I think it is most important that the patient's usual meal-times should never be upset immediately before an operation. The quantity of food allowed, and to some extent the actual kind of the food, should be carefully regulated, but the hour at which it is administered should not be altered.

Thus, supposing an operation (in a healthy subject) is to be performed at 9 o'clock in the morning, the patient should be left asleep, unless he awakes of his own accord, until at any rate 7 30, when he may be given his enema. Then, if he be accustomed to breakfast at about 8 o'clock, he may have a thin slice of bread-and-butter, or a small piece of toast, with as much tea to drink as he desires. He will then be able to settle down much more contentedly to his newspaper until the surgeon arrives. Tea is, I think, preferable to coffee, as the former is usually taken with considerably less milk than the latter.

If the operation be at 10 or 11 o'clock, he may have a similar breakfast, but with (say) two thin slices of bread-and-butter, if he wishes, or a somewhat larger piece of toast, provided his usual breakfast hour be 8 o'clock. If, however, he be accustomed to breakfast at 9, the amount of solid food should be as in the first case, unless the excitement of the approaching ordeal has caused him to awake earlier than usual, under which circumstances he may also have his small meal a little earlier

If the operation be arranged for 12 or 1 o'clock, he may be given an ordinary light breakfast at the usual time; meat, however, being avoided. A glass of water might, in this case, be taken with advantage at about 11 o'clock.

For an operation at 2 or 3 o'clock, the ordinary breakfast should be taken at the usual time, and a cup of hot clear soup or beef-tea at 1 o'clock, together with a little water to drink.

This same arrangement is suitable for a case to be done at 4 o'clock, except that in this instance a little toast, bread, or biscuit may be given with the soup.

If 5 o'clock be the appointed hour, the breakfast and lunch should remain the same, but in addition a cup of tea may be taken at (or a little before) 4 o'clock.

If 6 o'clock be chosen, the patient may have his ordinary breakfast, and then a light luncheon consisting of fish or poultry at about 1 o'clock, followed by tea with bread-and-butter at 4 o'clock.

Finally, a patient who is to be operated upon at 7 o'clock may partake of his usual breakfast, lunch, and tea, simply avoiding currant cake or anything rich at tea-time. No fruit of any kind should ever be taken on the day of the operation.

I need not carry this list further, as it is unusual to perform operations of the class I am now considering after 7 p.m., and I trust I have said sufficient to make it clear that more attention should be paid to the ordinary habits of the individual patient, in order to relieve him as far as possible from the discomforts of anaesthesia. I have mentioned definite hours for the meals (8 a.m. for breakfast, 1 p.m. for lunch, and 4 p.m. for tea) merely for the sake of convenience. If the individual patient under consideration be accustomed to take his meals at some other hours, these should be substituted for the ones I have indicated. An exception may be made in the case of breakfast if the patient should awake much earlier than usual on the morning of the operation. In this case, if he feels inclined for food at an earlier hour than usual, there is no objection to his having it then instead of later.

In private practice there is not the slightest reason for neglecting to devote the requisite amount of attention to each case in order that the patient's habits may be upset as little as possible, though the matter may be somewhat more difficult in a hospital; first, because some definite routine is far more necessary in a large institution, and secondly, because hospital patients are more apt to swallow lumps of solid food without sufficient mastication than are the people one meets with in private practice. I do not think, however, that these difficulties would prove to be insurmountable if a genuine attempt were made; and the additional mental and physical comfort of the patient would be ample compensation for the extra trouble involved.

OPHTHALMIC SCHOOL CLINICS IN THE COUNTRY.

By R. BEATSON HIRD, M.D., F.R.C.S.E.,

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School medical inspection cannot be regarded as complete unless the defective children receive the treatment they require. However interesting it may be to know the percentage of children in our schools who are suffering from various physical ailments, it is quite clear that the practical value must certainly lie in alleviating these. A nation's future depends on its children, and having found that many of them are physically defective, we must see that these are enabled to become healthy and useful citizens.

The treatment of defective school children has been occupying our attention for the past few years, and the question is still being debated as to who are to treat these children, and where the treatment is to be carried out. There are two classes of children to consider—namely, those dwelling in the towns and those who live in the country districts.

With regard to the town children, in some places the treatment is being carried out at the hospitals. At some of these special assistants have been appointed to cope with the much increased work, especially in ophthalmic departments, where the increase of refraction cases has threatened to strangle the routine work. Objection has been raised to the use of the hospitals for the purpose of treatment of these cases, though they are mostly drawn from that part of the population who rightly belong to the hospital class.

Another alternative to treatment at the hospitals is that of holding "school clinics," and paying medical men for undertaking the work. Such is being done in some parts of the country. It is of my experience in this matter that I wish to write. I do not intend to discuss the merits of one system as opposed to another. What the medical profession want to know is what is being done in different parts of the country, and how is it working with regard to all parties concerned. Then, perhaps, we shall be able to evolve some scheme which will work smoothly.

The question arises, What is to be done for the children in the country districts where there are no charitable institutions, and where cases demand special treatment? Of course, the children can be sent up to the towns to attend the hospitals. In the remoter country districts this has obvious disadvantages. Some parent or relative will have to accompany the child, thus adding to the expense. The greater part of a day has to be given up, often entailing a loss of wages to the parent. The expense in many cases is too heavy to be borne, and out of all proportion to the wages earned. There are other disadvantages, too, such as the "ticket system" and limitation of numbers, which are in vogue at some hospitals. In such cases the child runs the risk of not being seen after all. If the treatment can be carried out in these cases free

from many of these disadvantages, I think an effort ought to be made towards that end.

In the autumn of 1908 a voluntary association was formed in North Worcestershire, called the "School Medical Association." It was formed with the object of raising funds to undertake the medical treatment of the children found defective in the Bromsgrove District Council Schools. The success of the enterprise was largely due to the very energetic chairman. An appeal for funds was sent out, and on this was a statement with regard to the objects of the association, from which I quote the following:

This voluntary association was formed to follow up the education authority's medical inspection of elementary school children in the Bromsgrove and Lickey district. Up to now the authority has merely pointed out the existence of defects, and beyond informing the parents that defects existed which should be attended to, has taken no action as regards prescription or treatment, and it was found that many parents were—owing to lack of means and other causes—quite unable to take the necessary steps. The association deals more particularly with the treatment of defective eyesight, and the Worcestershire County Council have given permission for the free use of a room at the various schools for the carrying out of the oculist's work.

In the event of the parents not bringing the child for the initial examination, one of the association's visitors calls and endeavours to influence the parents.

The association considers cases where parents are unable to pay for the spectacles or drugs, and where thought advisable, make a grant towards part or the whole of the cost. Hospital notes are procured for any special cases that can be better treated at a Birmingham hospital, and drugs, etc., are supplied if required.

It is not the intention of the association to assume the responsibility which properly belongs to the parent, and where assistance is given towards the provision of spectacles they will be retained in care of the head teacher for the use of the children at school only.

The association now appeals for annual subscriptions, guaranteed for a period of three years, or for such shorter time as may be necessary, until the work may be provided for by the regulations of the local authority or the Board of Education, and invite your co-operation in the work.

It will be seen from the above that the association wished to remove certain of the disadvantages which I have spoken of, but intended that the parents should defray most, if not all, of the cost of drugs and glasses, the association paying the medical man for his services. This has worked well, and I believe over 80 per cent. of the parents have paid for the spectacles and drugs ordered. The necessary funds having been raised, a start was made late in the autumn of 1908, when the association appointed me as their oculist.

With a view to undertaking the treatment locally, I made a tour of the schools to see what accommodation there was for the work, and decided that it could be carried out there. For this purpose, permission was obtained to use the schools on Saturday mornings, when the pupils were not at work.

As only a few of the children attending the schools had been examined by the school medical inspector, it was decided to test the vision of all the children attending, and this was done by the teachers using Snellen's test types. A list was made of all children whose vision was not $\frac{6}{12}$. Children whose vision was better than this, if complaining in any way of their eyes, were also included. At the time of the examination the children attending these schools were as follows:

School.	Boys.	Girls.	Mixed	Infants.	Total.
Stourbridge Road C. S. ...	237	194	—	129	530
Catshill C. S.	106	110	—	102	318
Lickey End C. S....	—	—	195	88	283
Dodford C. S.	—	—	79	40	119
Sidemoor C. S.	—	—	—	128	128
Totals	343	304	273	487	1,378

Out of these there were found at that time 80 suffering from ocular defects, or 5.8 per cent. They were distributed thus:

School.	Boys.	Girls.	Total.	Percentage of School Total.
Stourbridge Road C. S. ...	12	15	27	5.0
Catshill C. S.	11	12	23	7.2
Lickey End C. S....	9	9	18	6.3
Dodford C. S.	—	4	4	3.3
Sidemoor C. S.	3	5	8	6.2
Totals	35	45	80	

With regard to these percentages, I should like to make a few remarks. Lickey End and Stourbridge Road Council Schools are modern brick buildings, well lighted, though faulty in arrangement in places, and with good ventilation and usual accommodation. They are well situated and have good playing grounds. On the other hand, the Catshill Council School is bad. Situated in the midst of a poor village, low, and beside a stream, it had a damp air about it in winter. The building of brick is small and totally inadequate for the numbers attending. The ventilation in consequence, especially in winter, is very bad, and the lighting, especially in the infants' school, insufficient. The cloak-room accommodation is too small, and the playing grounds suffer in like manner. It is not surprising, then, that this school should yield the highest percentage of ocular defects. It was decided that the school should be rebuilt over a year ago, but this has not yet taken place. The inhabitants of Catshill are nail-makers, and the standard of living very bad. The low percentage of defectives at Dodford is accounted for by the fact that they are all young children.

The arrangements made for the work were as follow: I attended at one of the schools at 9.30 on a Saturday morning. The number of children, whose parents had been notified, were in attendance at that time for examination and treatment. The refraction cases were first dealt with, homatropine or atropine drops being instilled. As each child had to be measured for the spectacle frames and a record kept of all particulars, for future reference, it was found that not more than 12 to 15 cases could be seen in a morning. While the cycloplegic was taking effect the cases of external diseases were examined and prescribed for, and any necessary instructions were given to the parents, who in many cases then left, returning later, if necessary, to fetch the children. I made it a rule to find out if any child was under the treatment of a local medical man, and always gave him the opportunity of undertaking the treatment where possible. All the children seen by me were of the poorest and bona fide hospital patients. For refracting purposes one of the smaller class-rooms was selected as a dark-room, and was rendered so by blinds and screens where necessary. For a portable lamp I used a Priestley Smith candle-lamp which I mounted on a telescopic metal camera tripod. This enabled me to put the lamp at any height desired. There was nothing to go wrong, and I found it worked admirably and gave quite an excellent light for refractive work. It was necessary to see some of the cases more than once, and in all eight visits were required to complete the work for that year.

I will now refer briefly to the defects found in these 80 children.

The sex incidence was: Males, 43.75 per cent.; females, 56.25 per cent. These figures correspond exactly to those given by Bishop Harman.[1] Of these, the majority required glasses—namely, 69, and they were made up as follows:

Hypermetropia	37	= 64, or 92.75 per cent.
Hypermetropic astigmatism	27	
Mixed astigmatism	1	= 5, or 7.25 per cent.
Myopia	2	
Myopic astigmatism	2	

The percentage of myopes is low. Harman[1] found that they formed 29 per cent. of the refractive errors. It has been maintained that the country children tend to be long-sighted and the town children short-sighted, and these figures help to bear this out. Of the 5 cases of myopia, 3 occurred in the Stourbridge Road School, which belongs to the town of Bromsgrove. The one case of mixed astigmatism was in a child of 4; of the other myopes, 1 was 10 years old, and 3 were 12 years old. With regard

to the ages of the children affected, 18 were 6 or under, and came under the division known as infants. The vision of 7 of the children was less than $\frac{6}{18}$.

The next most common condition was squint. There were 13 cases, and of these 10 were left, 2 right, and 1 alternating. In connexion with this it is interesting to note that the number of cases of anisometropia was 20, and the proportion of worse eyes was 4 right to 16 left. By the term "anisometropia" I mean all cases where one eye differed from the other by one dioptre or more, the worse eye being that with a higher degree of refractive error.

There were 5 cases of nebulae of the cornea. One was due to traumatism, 2 to ophthalmia neonatorum, 1 to phlyctenular keratitis, and 1 of doubtful origin—probably phlyctenular. The other conditions found were: Spasm of accommodation, 2; conjunctivitis, 4; blepharitis, 7; ankyloblepharon, 1; hordeolum externum, 2; heterochromia, 1; congenital cataract, 1; fundus disease, 2 (1 myopic); retrobulbar neuritis, 1; and nystagmus, 1 case (nebulae of cornea).

This year the work has been continued, the total number of children attending the above schools being 1,410. In addition two other schools have come into the scheme—namely:

Linthurst Church of England School with 94 children
Rubery St. Chads " " " 213 "

The total number of defectives treated this year has been as follows:

School.	Boys.	Girls.	Total.
Stourbridge Road	13	9	22
Catshill	—	7	7
Lickey End	1	4	5
Linthurst	2	1	3
Rubery	—	2	2
Totals	16	23	39

The number of visits made has been five. One was a tour of inspection of all the schools to see all the children who had been treated and the way they were progressing. It was extremely gratifying to hear from the teachers how well they were getting on. I may record the case of one of the school "dunces," a child who had given much trouble to the teacher, and who was regarded as a naughty and unmanageable child. A pair of spectacles to rectify the marked refractive error cured the moral and mental deficiencies and rendered the child an industrious pupil! How many children have suffered in like manner in times gone by when a pair of spectacles rather than the "rod" would have rectified matters! The number of glasses prescribed this year has been 39, and of these there were 5 cases of myopia as follows: Myopic astigmatism, 3; myopia, 1; mixed astigmatism, 1.

In addition there were: Squints 8, of which 2 were right, 5 left, and 1 alternating. Other cases were: Spasm of accommodation, 2; nebulae, 1; blepharitis, 1; conjunctivitis, 3; corneal ulcer of strumous variety, 2; fundus disease (tuberculous), 1; and epicanthus, 1.

With regard to the treatment of squint, the best had to be done under the circumstances. Some eyes were hopelessly amblyopic, and others greatly improved with glasses and education by occluding the good eye. The improvement was noticed when the cases were inspected a year later. Except in a few of the younger children, atropine ointment was not given for home use. The majority of the children were over 8. The scheme has worked smoothly, and there has been very little opposition from the parents, who, as a whole, took an intelligent interest in the work and willingly paid for the glasses and treatment. The chairman, Mr. Beesly, usually attended when I went over to do the work, and ably assisted me.

A letter from Mr. Hugh Thompson in the British Medical Journal of April 16th, 1910, showed that school clinics have been carried on in Woolwich on similar lines. That school clinics can be carried on successfully for ophthalmic cases I have no doubt, and they have much to recommend them for the country districts.

REFERENCE.
[1] Bishop Harman, *Proceedings of the Royal Society of Medicine*, May, 1909.

SUBMUCOUS RESECTION OF THE SEPTUM NASI.

By T. JEFFERSON FAULDER, M.A., M.B.Cantab., F.R.C.S.Eng.,

SURGEON TO THE THROAT HOSPITAL, GOLDEN SQUARE, AND CHIEF ASSISTANT, THROAT DEPARTMENT, ST. BARTHLOMEW'S HOSPITAL.

Submucous resection of the septum nasi is an operation of comparatively recent date, but is already in extensive use. Little information is as yet forthcoming as to the after-results. For example, does any regeneration of tissue take place in the septum after removal of the skeletal part? Opportunities of investigating this latter point do not often occur.

On July 2nd, 1910, amongst my other operations at the hospital was a man aged about 38, whom I had not myself previously seen. His complaint was nasal obstruction and "irritation," and the operation proposed was submucous resection of the septum. For this purpose his nose had been anaesthetized with cocaine and adrenalin hydrochloride in the ordinary way. On removing the packing

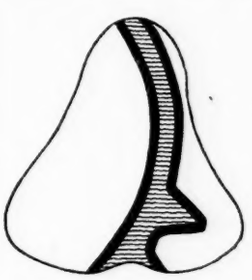

Fig. 1.

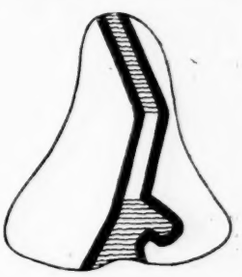

Fig. 2.

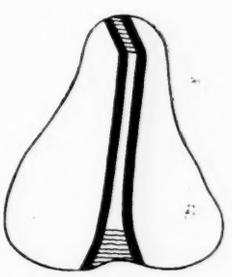

Fig. 3.

The thick black lines represent muco-perichondrium or muco-periosteum, the shaded areas bone or cartilage of the septum.

to make a beginning of the operation, I found a deviation of the cartilage to the left and a projection, also to the left, along the line of the ethmo-vomerine suture, preventing a good view of the interior of the nose on that side (see Fig. 1). The usual incision was made, and elevation of the muco-perichondrial flap begun and continued for about $\frac{1}{4}$ in. all along the wound. It was then seen that the cartilage was deficient, as though part of it had been removed at a previous operation, for the elevator was now between two layers of unresisting tissue (see Fig. 2), and also the appearance of the deep surface was not like that of cartilage. The separation was, however, continued as far back as the perpendicular plate of the ethmoid. After this separation, the most projecting part of the cartilage in front and above and the spine below were cut away and the wound closed. It seems clear that a partial resection had been done previously in this case. The patient stated that he had had several intranasal operations, and that after one of them, three years ago, he had worn a rubber splint in each nostril for some time. This latter operation must have been a resection of the septum. In that case

there are two points of interest: (*a*) the comparative ease with which the muco-perichondrial layers were separated from each other after nearly three years in contact—I came across only a few feeble strands of fibrous tissue crossing between the two flaps; (*b*) the entire absence of any regenerated cartilage, bone, or fibrous tissue beyond the weak fibrils above mentioned. This case also illustrates the occasional impossibility of making the septum hang truly vertical in the middle line without endangering the integrity of the bridge. When the deviation extends up to the very bridge of the nose it cannot be entirely corrected without removing the entire cartilage. A compromise has, therefore, to be effected, and the final condition is something like that shown in the diagram (see Fig. 3).

THREE BAD PROGNOSTIC SIGNS IN ECLAMPSIA.*

By WILLIAM FLETCHER SHAW, M.D.,
PATHOLOGIST, ST. MARY'S HOSPITAL FOR WOMEN, MANCHESTER; LATE RESIDENT SURGICAL OFFICER, ST. MARY'S HOSPITAL.

On no subject in obstetrics is there more modern literature than eclampsia. From wild guesses at its causation we have narrowed our conception of it down to a toxaemia, though the nature of this toxin, and even its origin, has still to be proved. Research in this direction now belongs to the chemist; for a clinician to attempt to elucidate the problem is mere waste of time, his results being worthless without a long special training. But the clinician can be of service in directing the chemist's efforts, by noting any special train of symptoms or the results from special lines of treatment.

The object of this short paper is to draw attention to three bad prognostic signs in eclampsia—namely: (1) A small amount of albumen in the urine; (2) a high temperature; and (3) *post partum* onset of symptoms—the first two of which I cannot find mentioned in the literature.†

Soon after it became my duty to attend a large number of cases of eclampsia, I was struck with the bad results in patients with these symptoms, and, as time went on, I was more and more convinced that these patients had a much greater mortality than the others. Knowing that our convictions are very often unconsciously formed from isolated cases rather than from the whole, I have now looked up the notes of all the cases of eclampsia admitted to St. Mary's Hospital in the years 1906-7-8, numbering 46 altogether, and find my observations much more strongly confirmed than I could have expected.

I.—*Small Amount of Albumen in the Urine during the Eclamptic Stage.*

For the purposes of this investigation I have taken 10 grams of albumen per litre of urine as the standard, any amount above that being considered large, anything below small. My reason for choosing this standard is solely that it seems a convenient one and one which gives a good cloud on boiling.

Of these 46 cases, 21 had a slight degree of albuminuria, 18 a large degree, and in 7 cases it was not recorded. The results of these cases were:

21 patients with *less* than 10 grams per litre; 13 died, 8 recovered—mortality 61.9 per cent.

18 patients with *more* than 10 grams per litre; 3 died, 15 recovered—mortality 16.6 per cent.

These figures show an enormous difference in the mortality. Is it not probable that we are dealing with two entirely different classes of cases, though they both have many of the same clinical symptoms?

II.—*A High Temperature.*

100° F. has been adopted as the standard, and only the readings noted which were taken during the eclamptic state; a high temperature during the puerperium, after the eclamptic period has passed, being necessarily of other origin.

Of these 46 cases, 26 had a temperature of, or above, 100° F., while 19 did not reach this temperature, and 1 was not recorded, as the patient died immediately after admission. The results of these cases were:

26 patients with a temperature *of* or *over* 100° F.; 15 died, 11 recovered—mortality 57.7 per cent.

19 patients with a temperature *below* 100° F.; 1 died, 18 recovered—mortality 5.2 per cent.

The results are even more striking than these figures indicate, as several of the 11 recoveries with a temperature of over 100° F., could really be discarded. In two of them the temperature only just reached 100° F. for one reading; in two others there was only a single rise just at the time of delivery; in one the patient had pneumonia and another was septic from a macerated fetus.

The patient who died without the temperature reaching 100° F. was moribund when admitted to the hospital, and died in less than two hours; so it is quite possible her temperature may have been raised before admission.

The temperature of these patients is recorded four-hourly. Several of the fatal cases had only a single rise during the eclamptic stage, but the majority had a progressive rise until death, and this sign warrants the very gravest prognosis. When these two symptoms, a small amount of albumen and a high temperature, occur separately, they are, as shown by these figures, bad prognostic signs; but when they occur together they give the very worst prognosis, while without either the prognosis is extremely good.

Of these 46 cases, 12 combined these two symptoms, while 7 had neither. The results in these cases were:

12 cases with *less* than 10 grams per litre and temperature *over* 100° F.; 12 died, 0 recovered—mortality 100 per cent.

7 cases with *more* than 10 grams per litre and temperature *below* 100° F.; 0 died, 7 recovered—mortality 0 per cent.

III.—*Convulsions Commencing after Labour.*

Very divergent opinions are held of the prognosis in those cases in which convulsions commence only after labour. Recently Johnstone[1] has collected statistics from many of the large clinics, with a varying mortality of 5.2 per cent. to 43 per cent. The author points out that the earlier the onset the milder the attack, and it is quite probable that in many clinics these mild cases are not received, so their mortality is really based on the severe cases. Of these 46 cases, in 8 only did the convulsions commence after labour, and of these only one recovered; in fact, this is the only recovery from this condition in four years at St. Mary's Hospital.

The motive of this short communication has not been an attempt to explain these phenomena, but simply to record them, to see if they are confirmed by the experience of other observers, and, if so, to give some further clinical data to direct the work of the chemical investigator.

REFERENCE.
[1] *Journal of Obstetrics and Gynaecology of the British Empire*, January, 1910.

* Read before the North of England Obstetrical and Gynaecological Society, May, 1910.

† Since writing this Galabin and Blacker's *Midwifery* has been published, in which attention is drawn to the bad prognosis with a high temperature.

THE formal opening of the chemical and physiological wing of the University of Bristol by the Pro-chancellor Lord Winterstoke will take place on Tuesday, November 15th, at 3 p.m.

AT the half-yearly meeting of the governors of the University College of North Wales, held at Chester on October 26th, it was announced by the treasurer that Sir T. Pritchard Jones had presented the new college with a great hall at the cost of £17,000.

WE understand that the authorities of the Heritage School of Arts and Crafts for Crippled Children at Chailey, Sussex, are finding themselves in difficulties, and require support. The school undertakes to educate both boys and girls, the object being so to train them as to render them capable of supporting themselves by hand crafts. The girls' school is a new building, and both schools have full recognition from the Board of Education; but the boys' school has been carried on in an old house which was only passed by the Board of Education as satisfactory during the experimental stage of the school's work. The authorities of the school are now faced by the necessity of transforming this old house at a cost of about £5,000. The honorary treasurers of the school are Lord Llangattock and Miss Alice Rennie, 36, Westbourne Terrace, Hyde Park.

SEVENTY-EIGHTH ANNUAL MEETING
OF THE
British Medical Association.

Held in London on July 22nd, 23rd, 25th, 26th, 27th, 28th, and 29th.

PROCEEDINGS OF SECTIONS.

SECTION OF OPHTHALMOLOGY.

CHARLES HIGGENS, F.R.C.S., President.

PRESIDENT'S INTRODUCTORY REMARKS.

THE PRESIDENT reviewed the changes in the practice of ophthalmology that he had known during his experience of forty years. In some branches there could be no doubt that they had made great advances both in theory and practice; in others the advance, if any, was small. Considering this second group, he instanced the grave diseases of trachoma and glaucoma. In respect of the first something more had been learnt of its histology and possibly of its pathology, but, save for the fact that the old name of "granular lids" or "ophthalmia" had been dropped for the more classical name of "trachoma," there had been no alteration in practice regarding it that was worth the name. Of glaucoma it could be said they had learnt a great deal of its pathology, but as regards its treatment he was sure that a "bad" iridectomy was now, as it was forty years ago, the very best and most successful operation; and by a "bad" iridectomy he meant one that left a minute fragment of iris in the scar, so that the wound never properly healed, but maintained constant leakage proportional to the tension of the eye. In other diseases, such as inflammation of the anterior uvea, there was little advance to record; these cases were as mysterious and intractable as ever, but he would not say much on this, as the subject was to be considered in a set discussion of the Section. In diseases of the lacrymal apparatus the only improvement in methods he could cite was the very general cessation of the practice of splitting up the canaliculi for every form of epiphora; it was a good change. The greatest advances had, without doubt, been in the realms of optical work. The treatment of squint had been revolutionized, correction of defects of vision was uppermost, and operations had become proportionately rare, with the best results.

In quite recent years their methods of testing colour vision had been improved greatly, thanks largely to the labours of Dr. Edridge-Green; but the matter was still the subject of inquiry, and it was without doubt a complicated one. The establishment of the universal use of the metrical system of measuring lenses had been of the greatest value and convenience in practice; younger members of the profession could scarcely conceive the worries in fractional calculations they had escaped by the substitution of dioptres for inches. Again, much had been gained by improved and standardized test types. The appliances for this work had been improved by the universal use of electricity; it was a boon not to be forgotten. Coming to operative measures particularly, he could not see very much change. To judge by the makers' catalogues, it would seem that every man invented instruments of his own, but the essential tools of their craft remained unchanged; Graefe's knife, the keratome, forceps, and cystotome were the basis of most modifications of cataract instruments. But in the introduction of the giant magnet of Haab there was a real gain, notwithstanding that it was an appliance for hospital work rather than private practice. In some fields of labour there had been a recrudescence of old operations. Sclerotomy for glaucoma bade fair to be fashionable once more, but it was no new thing. Bader and himself had practised it by incision and by the use of trephines; they mostly did it to relieve tension in blinded eyes, and the effects were undoubtedly good, but the practice had been overlaid by the fashion for iridectomy. Again, removal of the lens in its capsule was an old operation; it was described in Macnamara's textbook, and then mentioned as old. The speaker did not favour the method himself, but he had practised it rather by way of accident than purposely, and no harm had resulted; the human eye would withstand a lot of handling. In the treatment of syphilitic affections changes were to be observed; at one time the use of mercury was grossly excessive. Then came the inevitable reaction and its replacement by infinitesimal doses of potassium iodide; now their practice seemed to be rational and successful. In local anaesthetics a great gain had been obtained. The contrast to operating with badly administered general anaesthetics and the former risks to post-anaesthetic vomiting to their present invaluable cocaine was immense; it appeared to him to be the greatest advance of his time. Antiseptics had also done much for them, asepsis little, for the attainment of this in the conjunctiva was wellnigh impossible. Then, in the objective diagnosis of refractive errors the substitution of retinoscopy for the use of the direct ophthalmoscopy had been invaluable; he remembered the time when such a master of the refraction ophthalmoscope as John Couper was not ashamed to spend hours over a few cases, which to the surgeon of to-day would occupy but minutes. One other point was worthy of remark, that was the enormous increase in the number of ophthalmic surgeons. In his early days there were but a handful in London, now there must be more than a hundred. He hoped competition would have no ill effect upon their work. In this connexion he wished more power to the British Medical Association and an increase of its efforts to prevent abuses both within and without the profession.

DISCUSSION ON
THE FUTURE OF OCULAR THERAPEUTICS.

OPENING PAPER.

By G. A. BERRY, M.B., P.R.C.S.Ed.,
Edinburgh.

Local and General Measures.

IF we consider what has been the history of therapeutics in the past, we may well hesitate to predict what, in either a general or a special sense, it may come to be in the future. The title chosen for the discussion which I have the honour to introduce, if taken literally, would be absurd. The obvious idea which underlies it is: How far do our knowledge, our aspirations, and the directions given to our scientific and practical views of to-day, indicate the direction in which any real advance may be expected to take place in the near or more remote future?

First, I think we must remember that practical therapeutics, unless it advanced to a stage which present experience would hardly lead one to conceive possible, can never be brought within the limits of very definite rules. A great deal will always depend upon the individuality, the temperament, the judgement, and the resourcefulness of the medical man. I do not say his experience, because, according to the degree in which the qualities to which I have referred are developed, either alone or combined, what he calls his experience may be of little or no use, or may be of the highest value to himself and to others. Then there is almost infinite variety met with in the temperament, the toleration, vulnerability, diathesis, and so on of the patient he is called upon to treat. Even in eye affections, the diagnosis of which is usually attended with comparatively little difficulty to the specially trained surgeon, and a better insight into the pathology of which is constantly being acquired more and more as science advances, one has to subordinate the treatment of the disease to, or at all events to correlate it with, that of the patient. How much scope all this leaves, and always must leave, for differences in treatment, is familiar to everyone whose daily work is chiefly clinical.

The progress of science—in the domains more especially of physiology, pathology, and bacteriology, but also in those of chemistry and physics—is constantly affording new *indications* for treatment. As these sciences advance improvements are also suggested in the methods of carrying out, or at least explanations are afforded of the effects of, a therapy which has hitherto rested on a more or less purely empirical basis. Nevertheless it is

a common, and I believe a true, remark that the main advances in treatment are those which clinical experience has established. The mere fact that some scientific discovery has led to a change or a modification in the treatment of some particular affection does not ensure such new treatment being any better than, or even as efficacious as, other methods which do not seem in the same way to comply with the conditions which a more accurate, or presumably more accurate, acquaintance with the pathology of that affection would seem to demand. The clinical experience of many, often indeed a prolonged experience, may be necessary to demonstrate its utility or its failure.

There are many reasons why an indication afforded by scientific observation and experiment should therapeutically prove disappointing. If it is afforded by some new discovery in physiology, it is often found that it will not adapt itself to pathological conditions. Not infrequently a chemical or physical interference with the functions of the healthy organs, which is found to produce most marked stimulating, or sedative, or other effect, may, when applied to the same organ in a diseased state, have little effect, or its effect may even be one that is more or less decidedly detrimental to the course of the disease. And after all, although an advance in the knowledge of the pathology of any disease will often evoke a whole train of ideas suggestive of how that disease may be *rationally* treated, it is comparatively seldom that a conspicuous advance on the methods of treatment established by practical experience is the result. Even the definite antiseptic properties which some particular substance may have been experimentally demonstrated to possess, as a destroyer of the specific micro-organism that is known to cause more or less characteristic and localized symptoms, do not necessarily prove it to be a suitable remedy as a local application where the symptoms show themselves. The problem of attacking the micro-organisms in the tissues, and in presence of the altered conditions caused by local tissue reaction, is one of greater complexity than can usually be simulated in a laboratory experiment.

And when we come to the application of toxins, antitoxins, serums, vaccines, etc., for which there appear to be scientific indications, it is perhaps less extraordinary that the very complicated, difficult researches which give rise to inferences as to the possible therapeutic value of such substances should meet with even greater practical discrepancies. The indications are themselves often less apparent, and it is hardly to be wondered at that the outcome, so far, of these newest resources of therapeutics should be far from commensurate with the expectations which have been entertained.

Our art, which at one time existed on a firmer basis than did the accurate scientific knowledge of the time, now lags, and will no doubt continue to lag, behind our science. Yet it cannot well be doubted that even as an art advance is real and steady. However true it is that clinical experience must in both cases determine the value of any kind of treatment, and that accurate clinical observation is, in therapeutic matters, of an order of complexity and difficulty exceeding that of other inexact experimental sciences, it is surely also true that treatment initiated in response to the indications which science affords, is on a surer, and more necessarily progressive, basis than that which owes its origin to blind empiricism.

If we exclude what may be termed operative or surgical, and also optical, treatment, our resources, so far as the treatment of eye disease is concerned, may be considered under the two heads of *local* and *general* treatment. Both are probably older than history. Local medical treatment, at any rate, was practised by the Egyptians from the earliest times of which we have any record, and general treatment was apparently resorted to in India and China long before it became the main sheet anchor of Hippocrates and his school.

Of local agents it is curious that so many, even in their original form, and still more in the simplified and purer forms which chemical advance has made possible, have survived the test of time. As regards others, there has, for example, come to be, and as the result of the modern knowledge of bactericides, a more extended and on the whole a more rational use of antiseptics.

Mydriatics and miotics, especially the latter, have comparatively recently come into prominence as topical agents, while local anaesthetics, analgesics, vaso constrictors, and vaso-dilators are almost entirely of recent introduction. Still, it can hardly be said that as yet the use of drugs having these properties as local applications has been established on a basis which admits of no difference in practice. Their various physiological actions, it is true, are understood, but their therapeutic uses are still in varying degrees insufficiently established to preclude their being improperly used or needlessly resorted to. An abuse of mydriatics and a too prolonged and too powerful application of local antiseptics seem to me to be not uncommon in practice.

Although the future may bring about a change in the particular mydriatics and miotics used—for example, possibly, in the more general substitution of the alkaloids themselves for their salts—it is at present hardly conceivable that the local application of drugs having their properties can ever fall altogether into disuse.

Much, however, remains to be done with regard to suitable and efficient local antiseptics. It is somewhat remarkable that, with the number of such substances which are already available, the oldest should, to a great extent, still appear to hold the field. Corrosive sublimate, which was in use long before its antiseptic properties were dreamt of, and boracic acid, one of the first substances known to have antiseptic properties, and to be used on account of these properties as a local agent in eye affections, appear still to be the prime favourites.

It seems not improbable that a selection of the most suitable antiseptics as bactericides for the particular micro-organisms which can now often be recognized as the cause of the symptoms observed, may become possible to a greater extent in the future than is the case to-day. It has, however, to be remembered that the bactericidal property of an antiseptic is not its only, perhaps not its chief, value as a local application. It is possible, therefore, that the combining of different antiseptics may in practice turn out to be not only not superfluous, but of real advantage. It is only recently that the superlative efficacy of zinc preparations in diplobacillary conjunctivitis has been established. Apart from this, there has been no further differentiation which has received general recognition. Surely in future we may look for some progress in this direction.

One point, of great importance as regards local applications to the eye, is that, generally speaking, one is apt to be comparatively ignorant as to the course which some affections would take in the absence of local interference altogether. It is not so very uncommon to hear from a patient that he got well after giving up everything in the way of treatment. Again, one may fail to appreciate the very great differences which exist in the virulence of the same affections, according to individual and other circumstances, which may be of influence in this respect. I need only remind you of the differences which are met with in cases of conjunctivitis and of serpiginous corneal ulcer. Even when those affections owe their origin to the same micro-organism, great variations occur in their persistence and in their destructive tendencies, apart altogether from the influence of any local treatment which may be adopted. Up to a certain point—that is if the tissues are able to overcome the insult caused by the particular irritant—the more acute and more pronounced their reaction, the more rapid and more effective is the natural cure. Again, the effect produced by the same micro-organism is subject to variations within wide limits. This may depend either upon differences in their virulence or upon differences in the soil which they attack.

These facts contribute greatly to the difficulty that exists in correctly appreciating the effects which are obtained by various local applications. They call for the greatest caution before arriving at any sweeping conclusions as to the value of any particular method of interference. They also, however, point in another direction and in one which, in the course of time, has received fuller attention and appreciation—namely, the necessity for studying, aiding and intensifying so far as possible, the curative action of Nature In this direction we have already seen advance made. There can be little doubt that further important advances will be made in the same direction in future.

In connexion, then, with ectogenetic poisons: we now re-recognize the indication for destroying the micro-organisms,

by which many are elaborated, as well as for fostering the natural tissue reactions, and for supplementing the antidotes and antitoxins which such tissue reactions bring into play.

Except as regards cauterization our antiseptic measures generally fall short of the requirements indicated. They do so because of their irritative, more or less destructive effects, and because of their failure to penetrate sufficiently deeply into the tissues to reach the organisms which they are meant to attack. Some of them probably fail, too, in rather arresting than stimulating the supply of the active combatants which are provided by the resources of Nature.

Cauterization, though capable of reaching and destroying the germs, only does so at the expense of the tissues in which they are located. It is only justifiable, therefore, where these tissues are necessarily lost, as the result of the intensity of their reaction, or are doomed in any case, in the absence of interference, to invasion and destruction. The justification in the latter case being similar to that which, in a threatened conflagration, would lead to the destruction of property surrounding the area on fire, in order to limit the loss which must otherwise be inevitable.

Greater penetration and less general irritation by the antiseptics employed is, then, a direction in which much is called for and not unlikely to be eventually attained. In two ways some steps have already been made towards complying with these indications—namely, by ionization and by subconjunctival injection.

In ionization we appear to have a useful means of accelerating and amplifying diffusion. The efficacy, on the other hand, of subconjunctival injections seems to depend, to a great extent at least, if not mainly, on other factors. Both methods of treatment are capable of greater precision than they have as yet arrived at. They may be expected to develop and figure more largely in the ocular therapeutics of the future.

As regards serum and vaccine therapeutics, etc., as to which no one can doubt that the future has much in store, I do not intend to say much, as that part of the subject for discussion will be opened by Dr. Earle.

A great deal has been done in this direction already. It may, indeed, be looked upon as the main direction which is being followed by the newer therapeutics. Comparatively little result has, however, followed, so far, the local application of these substances. The methods of treatment at present may be divided into the specific and the non-specific, or so-called para-specific.

The latter, which have been mainly advocated by Darier and Deutschmann, whose conclusions are, I fear, more often dictated by an enthusiastic credulity than by critical insight, have so far received little acceptance or support by those who have been induced to give them a practical trial. As regards specific serums, we may admit, I think, that good results not infrequently attend the use of Koch's tuberculin T.R. and of diphtheria antitoxin; while those got by graminol, pollantin, and jequiritol have been at least encouraging.

Turning now to the means employed for increasing the resisting powers of the tissues. In addition to the old-established application of dry or moist heat, we have at present subconjunctival injections of physiological saline solutions or other substances, or of air, paracentesis of the anterior chamber, and dionine.

It is curious that the old and purely empirical practice of paracentesis should, in the light of modern knowledge as to the characteristics of freshly formed and rapidly poured out aqueous, have received a scientific justification. The advantages, too, of locally induced congestion have long been known, and the present interest in this lies more in the better knowledge of the causes which make it valuable in treatment. As to dionine, which, on the other hand, is a recent introduction, though certainly not without use, I am personally unable to agree in ascribing to it the advantages which have been claimed for it.

There is some reason to believe that subconjunctival injections may stimulate the outpour of protective substances from the surrounding blood vessels. In addition, therefore, to their effect of scattering, and thus possibly aiding in the absorption of inflammatory products, and of admitting, as I have already mentioned, of the better penetration of antiseptics into the tissues, they have other effects which as yet are only imperfectly known. As a method of applying antisyphilitic remedies, I believe the value of subconjunctival injection has been greatly exaggerated.

Of other methods of local treatment which may be referred to we have electricity, *x* rays, radium, and massage.

As regards electricity, I must confess to the utmost scepticism as to any good having ever been done, in any eye affection whatever, by the use of either the continuous or interrupted electrical current, applied in the ordinary way. The possibility of any advance in this connexion taking place in the future is certainly not, I should say, foreshadowed by any benefit conferred in the past. Yet there may, no doubt, be much to learn of the influence of electrical currents on vital phenomena, so that an expectant attitude may not be unreasonable. Electrolysis, though of limited applicability, has, however, proved of undoubted service. Something, too, may be expected of iontophoresis.

It is, perhaps, too soon to attempt to define the sphere of radium. It seems, at least, to have the advantage over Roentgen-ray treatment of being readily and more safely circumscribed in its action. The future will, no doubt, see an extension of its application in the treatment of ocular disease. There will, at all events, be greater opportunities of forming an opinion as to its value and limitations.

From all time, so far as there are any records, diseases of the eye and its surroundings have been treated not only locally but *generally*. It is curious how, at different times and in different schools, the relative merits of the two kinds of interference have been differently estimated. While many of the local measures employed of old have survived—at least, in some more simplified form—this cannot with equal truth be said of general treatment. Excessive purging, bleeding, blistering, the use of emetics, the carrying of mercurial treatment to the extent of seriously injuring the system generally—these have all but disappeared. Sweat cures are also not so frequently resorted to as formerly. Perhaps this is a mistake. I am not sure that they may not figure again in the future more largely than they do at present. Are we, however, much further advanced since we have been led to adopt less drastic measures? I think we see more clearly what is to be aimed at. The indications afforded by present-day pathology are more precise, and we may at all events expect to achieve more satisfactory results in the future. So far there is not much to congratulate ourselves upon. The old methods of depletion, though empirical in their origin, were found to be of value. Hence it was, no doubt, difficult to escape overdoing them. In the natural reaction that has followed some may possibly be too little used. Whilst bleeding and purging, and even the more localized depletions by counter-irritants—such as setons and blistering, or by leeching—have probably had their day, sweat cures, as I have already said, are not likely to be altogether abandoned.

The poisons which may have a selective affinity for the tissues of the eye, and which may have been weakened by the old methods of depletion when not carried too far, are more and more likely in the future to be attacked by antitoxins or by methods which may increase the resistance of the tissues themselves to the toxins. The era of serum, vaccine, antitoxin, and cognate treatments may be said to have only begun. What is done in this direction to-day is insignificant compared with the possibilities which the rapidly accumulating discoveries in pathology seem to open out for the future. To take one example: A three or four years' experience of the now popular sour milk cure has gone far to convince me that endogenetic toxins are not only factors of no little importance in connexion with ocular manifestations of disease, but that much may be expected from a more complete knowledge of how they may be counteracted or prevented from originating.

The future advances of therapeutics generally will necessarily, too, be in great measure available in the treatment of diseases affecting the eye. We seem to be within measurable distance of time of being able to arrest the progress of tuberculous lesions, and may even hope eventually to learn the means of preventing their occurrence altogether. Syphilis and cancer may also, quite conceivably, yield to curative and prophylactic measures which the future has in store, and perhaps it is not too Utopian to think that the day is not so very far distant

when alcoholism as a factor in disease will be practically unknown.

Other factors, which at the present time are not prominent or even non-existent, may possibly arise to take the place of the four potent ones to which I have just referred when these have been weakened or overcome. But as to any new factors, we may say, "Sufficient unto the day is the evil thereof." Again, a more extensive knowledge of the influence of climatic conditions may lead to an extension of the treatment of eye diseases at baths and other health resorts.

As regards operative treatment, it would be rash to say that we have reached finality. Yet it is difficult to foresee what may be the direction of future advance. At the present moment various attempts at supplementing or supplanting the operation of iridectomy for glaucoma are being made. These aim at providing a safety valve for the escape of aqueous from the eye, by means of a more efficient cystoid cicatrix than could with any degree of certainty be obtained by older methods of operating. A more prolonged experience of the efficacy of these modifications in operative procedure is required before their value can be gauged with certainty. This is, however, also possibly a line in which there may be future advance.

One would fain hope, too, that some operative means may be devised for checking the advance of detachment of the retina, so that complete blindness may not be such an almost constant result as it is at present. For a number of years—ever since the publication of Deutschmann's first results—I have in my own practice adopted the plan of repeated double puncture, which seemed to me the element in his plan of treatment from which some good might possibly be got. Though far from being brilliant, the results have occasionally been encouraging, at least in what seem to be suitable cases, namely, cases of extensive detachment in individuals not beyond middle life. They have been encouraging, I mean, in regard to the permanency of the effect produced, because it has always to be remembered that there is usually some temporary improvement got just from lying in bed for some weeks. Perhaps there is more reason to hope for future advance rather in the prevention than in the treatment of retinal detachment, by the discovery of means which will prove prophylactic as regards the condition of the vitreous and choroid which precedes the actual detachment.

Another direction of advance, though not, perhaps, one of very wide possible scope, is that of preventing blindness from neuritis as well as from direct pressure on the optic nerves or tracts. This is a line on which brain surgery is advancing.

In one other important direction there can be little doubt that ocular surgery will some day triumph—namely, in the permanent improvement of defective sight caused by corneal leucomata alone. When this can be done, the sphere of usefulness of the ophthalmic surgeon will be materially widened.

In the very fragmentary and desultory manner in which I have ventured to introduce the subject of discussion, I have purposely avoided anything of the nature of a personal advocacy of any method of treatment. To others who may take part in it other lines of advance in treatment than those to which I have briefly referred—advances which I believe may reasonably be expected to be made sooner or later by complying with the indications of science—may suggest themselves. There are, of course, many other directions in which one would like to see advance made, and in which, one is fain to think, revolutionary changes must come in the course of time. But such as I now have in view do not appear to me to come so directly within the scope of our subject as indicated by its vague title. They are the cases in which, while improvement in treatment is more or less desirable, there are as yet no indications afforded as to how it is to come about. I shall, in conclusion, take one example illustrative of this kind of case—namely, the cure of cataract without operation. Although claims have been made by irresponsible and ignorant people unconnected with our profession, and even by some unprincipled members of the profession itself, that such a cure is possible, I am convinced that these claims have never been in the smallest degree justified. Yet I do not consider that the nature of cataract makes it in any way inconceivable that it may eventually be possible to arrest it at an early stage, or prevent its occurrence altogether. There is, however, nothing yet known which affords any indication as to how this may be done.

DISCUSSION.

Dr. J. W. H. Eyre (London) took up the consideration of "bacterial produces," saying scientific therapy depended upon accurate diagnosis; it was hopeless to expect improved methods of treatment without this as a basis. Take, for example, the well-known conditions of ophthalmia neonatorum; this was not a specific entity, but one that depended on the infection of several organisms—the gonococcus, streptococci, staphylococci, and others. Clearly any specific treatment needed a particular diagnosis. There were two groups of bacterial remedies—antitoxic serums and vaccines. He could not recognize local and general applications. What they did was to imitate Nature so far as possible. Their methods were the outcome of observations of general disease produced experimentally, of a close study of the course of infection, and of the production of antibodies directed against the invading organism. Careful inquiry showed that antibodies were specific; Nature was very strict in her methods in these infections, and one antibody would not serve against an alien organism. Hence the primary necessity for accurate diagnosis. Serum-therapy seemed to have only a limited applicability. There was one undoubtedly valuable serum for diphtheria, and it was of value in diphtheritic conjunctivitis. Of other serums little good had come—at least, in eye work. The antipneumococcic serum was not antitoxic, but antibacterial; it had proved of some service. But in his experience pneumococcic affections of the eye were uncommon in England, though more common on the Continent. One of the difficulties of obtaining specific serums was the inability to immunize animals of sufficient size. Vaccine-therapy seemed to present a much more fruitful field. Its object was to provide a stimulus to the natural processes, to provoke the production of antibodies within the affected subject in larger quantities than were naturally formed. They obtained cultures of the invader, destroyed them by heat, standardized the preparation, and injected known quantities of the vaccine into parts of the body distant from the original infection. The reaction caused a local production of antibodies, which, circulating throughout the body, exerted a beneficial effect upon the distant lesion. Of those most useful in eye work he would cite the old tuberculin T.R. of Koch, which they had only recently learnt to use in a proper manner; staphylococcal and streptococcal vaccines had proved of great value, in direct contrast to the inutility of the serums. Gonococcal vaccine had proved of value in iritis. The applicability of tuberculous vaccine was increasing, and he looked to it to afford a better means of treating tuberculous conjunctivitis than any surgical method. Lastly, he dealt with the relations of the ophthalmic surgeon to the bacteriologist. Harm was done by isolated individual action, for each failed in a knowledge of the other's line of work. To obtain the best results they must work hand in hand: the surgeon to observe the clinical aspects of the case, the bacteriologist to carry out the specific diagnosis and the indications for specific treatment.

Mr. Angus MacGillivray (Dundee) said he thought that Dr. Berry was rather inclined to be pessimistic of the prospects of advance. He regarded the advance in bacteriological methods of diagnosis of the highest promise—for example, in the case of corneal ulcer their action should be entirely determined by it; if the pneumococcus was found it was an indication for the immediate use of the cautery; but if the diplobacillus was the infecting organism, then the cautery was injurious and zinc became their sure weapon. Again, in conjunctivitis the finding of the Koch-Weeks bacillus indicated an inflammation that would speedily depart even without treatment; but the diplobacillus would keep up inflammation for long periods unless effectively treated with zinc. Fibrolysin injection he had found of great service in reducing scars of old interstitial keratitis; in this he disagreed with Dr. Berry. But he was entirely in accord with him in his strictures on the use of electric currents. The co-operation of bacteriologist and eye surgeon was likely to be of great value; but they should not hasten to discard all the laboratory expert

condemned—for example, he found argyrol of value in ophthalmia neonatorum, notwithstanding its feeble bactericidal power.

Dr. R. A. Reeve (Toronto) held that the efficacy of dionine had not been fairly tested unless the powder itself was used in direct application after the use of cocaine. In acute iritis with synechiae solutions of dionine might prove useless, but the powder produced speedy and gratifying effects; they must not be afraid of a marked reaction; the best result followed it. Again, he questioned the pessimistic view of electricity; high-frequency currents had been of service to him. Also fibrolysin injections had been followed by resolution of vitreous opacities responsible for detachment of the retina with resultant improvement in field and acuity of vision.

The President remarked on the use of electricity in early optic atrophy; he saw no justification in it—it was rather like flogging a dying horse.

Dr. G. S. Ryerson (Toronto) said he had had gratifying results in the use of radium in cases of epithelioma of the conjunctiva; these were cases that spread to the surrounding tissues, and incision did not secure cure without great loss to the tissues. Naevus of the lids also yielded readily to radium.

Mr. Hill Griffith (Manchester) said he had had the most gratifying result from the use of tuberculin in cases of tuberculosis of ocular and palpebral conjunctiva; complete cures had been obtained.

Mr. A. W. Ormond (London) referred to four cases of tuberculous periostitis of the orbit in which there was proptosis of the eye and loss of vision. In all cases part of the mass had been excised and proved to be tuberculous. Tuberculin injections had been used with unvarying improvement of the conditions. In two cases complete cure had been obtained and the other two were improving more slowly.

Mr. Leslie Paton (London) said that in the treatment of detached retina it was important not only to get the retina back into place but to fix it there. To lack of this, methods that at one time promised fair were eventually disappointing. He recommended the use of the cautery for perforating the sclera and retina, and the attempt to draw the retina into the lips of the wound. This method presented an ideal means of sealing the retina to the scleral cicatrix.

Dr. G. Montague Harston (Hong Kong) confirmed the beneficial effects of argyrol in 25 per cent. solutions in the treatment of ophthalmia neonatorum. The use of powdered dionine he considered too drastic; not only was the reaction severe, but there had been in one case he had seen a severe reaction of the patient against his doctor! He confirmed absolutely the conclusion of the President as to the uselessness of high-frequency currents, or any other electrical current in optic atrophy. In one case, however, he had seen some success from intramuscular injections of succinamide of mercury, but it was rash to draw conclusions from one case.

Major H. Gidney, I.M.S., considered dionine of great value in corneal opacities; the chemosis it caused need not alarm one. Fibrolysin he considered, after an extended trial, useless. He emphasized the necessity of restricting the use of the cautery to the advancing edge of pneumococcal ulcers of the cornea, and leaving the central portions, which had been shown to be free of organisms, untouched.

Dr. G. A. Berry, in reply, said that in regard to the possibility of cure or retardation of optic atrophy, he believed that those cases in which improvement was believed to have been effected by treatment were really retrobulbar neuritis and of the same order as those described by von Graefe in 1850 as amblyopia *sine causâ*.

THE USE OF CARBON DIOXIDE SNOW IN EYE WORK.

BY

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AND

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Amongst the newest of new remedies for external diseases is the application of carbon dioxide snow. Of its use in cases of capillary naevus there have already been put on record many cases; the extension of its use to papilloma and rodent ulcer is of quite recent date, and, so far as we know, the notes we present to you of its use in trachoma are the first records of the kind.

Carbon dioxide snow was first made use of for surgical purposes by Pusey, a dermatologist of Chicago. He used it successfully for the treatment of naevus; it was used in this country by Morton in June, 1909, at the London Hospital, and a paper was published concerning the usage in the *Lancet* of December 4th. The efficacy of this caustic in naevus can be gauged by the fact that one of us (E. R. Morton) has treated over 200 capillary naevi with 90 per cent. of cures at the first application.

The *rationale* of the usage of the snow is easily understood. Intense cold is as destructive of living tissues as is heat, and after the withdrawal of the destroying influence the destroyed part is exfoliated by the reaction of the surrounding undestroyed tissues. This puts briefly and somewhat crudely the maximum action of the snow. But its action need not necessarily be destructive of the tissue to which it is applied, for the duration of its application, and therefore of its action, can be regulated to a nicety, so that every degree of reaction can be obtained—from a scarcely detected blanching at the point of contact, with a subsequent sign of reaction in a delicate blush, to the complete freezing of the tissue and its almost instantaneous death.

Mode of Preparation and Use of the Snow.

Solid carbon dioxide is easily prepared. The gas can be procured in plenty, thanks to the aërated water industry. The compressed gas is sold in iron cylinders; it is cheap and clean. No other appliances are necessary for the production of the snow than a cylinder of gas, a towel, a cork, a brass tube, and a ruler that will fit into the tube.

The towel is rolled loosely around the ruler, the ruler withdrawn, and the resulting tube of towelling closed at one end with the cork. The towel-tube is held mouth-up beneath the vent of the gas cylinder, which being opened emits the gas into the tube, wherein a large deposit of snow appears. This is removed from the towel, broken up, and then shaken into the brass tube and rammed down with the ruler. To make the snow pencil quite hard it may be hammered forcibly into the tube. Then the pencil is pushed out from the tube by the ruler. The snow pencil can be handled safely or covered with lint; its extremity can be cut with a penknife to any desired shape. The hard pencil will gradually dwindle as it reassumes the gaseous form, but it will last for effective use from one to two hours.

In treating new growths the pencil is shaped to the surface to be treated and then pressed down on the tissue for about forty seconds; this is sufficient to completely freeze the tissue. In treating trachoma the lid is everted and separated from the globe by a non-conducting spatula. Then the pencil is lightly pressed down upon the portion of the conjunctiva selected for treatment for from fifteen to thirty seconds—for the shorter time at the beginning of the course, the longer time after a graduated increase with each weekly sitting.

In the use to which carbon dioxide snow has been put in the cases we now record these two extremes of effect have been made use of: In the case of trachoma the action is of a delicate order, and is used to promote a well defined shock to the tissues with a subsequent brisk reaction; no actual destruction of the tissues by the snow is permitted. In the case of papilloma and rodent ulcer, an action of the snow just short of its full extent is obtained, to ensure the death of the new growth without damage to the surrounding healthy tissue. In trachoma the duration of the application at each sitting is short and repetitions are frequent; in the case of new growths one full application is sufficient to obtain a cure when the growth is of small size.

Treatment of Papilloma and Rodent Ulcer.

A. G., female, aged 67, seen March 23rd, 1910, in the ophthalmic department of the West London Hospital with a large papilloma of the left lower lid. The growth was separated from the lid margin by 2 mm. of healthy skin. The tumour was as large as the half of a full-sized green pea, measuring 4.5 to 5 mm. in diameter, and raised 4 mm. from the skin level. Its surface was hard, harsh, and file-like. At first it was supposed to be a "horn" growing from a sebaceous cyst, but on detaching the hard surface it was proved to be a true papilloma. The old lady demurred to excision, so she was sent to Dr. Morton for treatment with the snow. The next day 40 seconds' application was made, and again another of 15 seconds a fortnight later. Three weeks from the date of her first attendance the scar was healed without puckering; indeed, a faint redness alone indicated the site of the destroyed papilloma.

Four cases of rodent ulcer have been treated by Dr. Morton. Of these two are private cases and are not available for publication; the other two are West London Hospital cases, one diagnosed by Dr. Abraham of the dermatological department, the other by Mr. Etherington Smith, one of the assistant surgeons.

J. E., female, aged 42. Rodent ulcer on the right temple, rather larger than a sixpenny piece. Duration six years. Treatment with the snow for 40 seconds on April 29th. The one application effected a complete cure.

H. A., male, aged 65. Rodent ulcer on side of nose. Roughly the shape of a parallelogram, measuring ¾ by ⅝ in. Treatment with snow for 40 seconds on June 29th. To-day the surface is completely healed, and one can scarcely believe that such a thing as a rodent ulcer has ever affected the skin.

Treatment of Trachoma.

A. C., male, aged 29. Seen at the West London Hospital January 12th, 1910. Complained that his right eye was "bleary"; it had been troublesome for several months. Examination showed roughness of the upper part quadrant of the cornea and invasion of new vessels, in fact pannus. Eversion of the lids disclosed a full-blown trachoma in each eye; the conjunctiva of both upper and lower lids was papillated, and there were numerous follicles, particularly over the upper tarsi. On that day, January 19th and 26th, the conjunctivae were rubbed with bluestone. The man felt the pain of this application greatly, and said it incapacitated him from his work. He was then treated with carbon dioxide snow, beginning on February 2nd; applications have been made each week with a few intervals. The time of application was at first 15 sec., then 25 sec. No pain was felt at the time of the application, and there was no "after-pain" at first, but with the increase to 25 sec. he complained of pain setting in twelve hours after, "an intermittent aching and shooting with watering of the eyes." His wife said "the eyelids swelled up and got red as though he were going to have a black eye." Subsequently 15 sec. was given and the time increased by gradual stages until he now has 30 sec. without experiencing any after-discomfort. No other treatment has been given save a lotion for home use, for some time copper sulphate drops, but for the longer time boric lotion only, and more as a "placebo" than by way of effective treatment. The trachoma is now scarring steadily and evenly; the pannus is quite gone.

L. W., a Jewess, aged 11. Her mother said her eyes had been bad several years. There was well-marked trachoma of each eye, with papillae, follicles, some early scarring, and pannus of the right eye. She was treated with the snow from the outset. Beginning with 15 sec., she now has 30 sec. once a week; but for a lotion for home use she has had no other treatment. The condition of the lids shows considerable improvement. At no time has she complained of any pain either at the time of the application or subsequently.*

Two other girls, both aged 11, inmates of an orphan home, with fresh papillary trachoma and a sprinkling of follicles, are now under treatment. At the date of the first examination there was some gummy discharge, so the lids were painted with silver nitrate; subsequently the treatment with the snow was begun.

On this short experience of the use of carbon dioxide snow in the treatment of such a chronic disease as trachoma we are not disposed to wax enthusiastic. When it is proposed to put forward a new treatment for a disease which, when it is full grown, takes on an average two years in which to effect a cure even with the most established form of treatment, and when that disease is one which has been treated in an innumerable variety of methods from time immemorial, we must require that the new form of treatment should be investigated with the utmost caution, and that any preliminary announcement of it should be made with all reserve. With this in mind we are content only to state the facts of the case as they have been seen by us. If its effects prove to be only half as good as those now obtained by the use of blue stone, then it has a future before it; first because it is painless, secondly because it is simple and cheap. Of the use of the snow in cases of new growth of the skin we can speak emphatically. The results are truly phenomenal, indeed they need to be seen to be believed.

* The cases were shown to the Section on the succeeding day.

DISCUSSION.

Dr. G. H. FINK (London) commented on the use of carbon dioxide when in solution in water. It had been proved by Hankin of Agra to be destructive of micro-organisms.

Dr. BERRY (Edinburgh) said he had been much interested to hear of carbon dioxide snow. As a manager of the Edinburgh Infirmary he looked on new remedies from an economic point of view, and the heavy expense they had anticipated by the need of securing radium had been obviated by the advent of the snow.

Dr. REGINALD MORTON (London), in reply, said that in addition to what had been said in the paper just read on behalf of Mr. Bishop Harman and himself he would like to add that even now, after having made over 1,000 applications with solid CO_2, the way in which papillomata, naevi, and rodent ulcer disappeared after this treatment continued to surprise him. He would not have believed such results possible had he not seen them himself. He did not ask the members of the Section to take his word, but rather advised them to use CO_2 in suitable cases and judge the result for themselves. He did not consider that CO_2 had any "selective" action on abnormal cells any more than he believed such to be the case with radium or the *x*-ray tube. In treating a rodent ulcer, for instance, provided the application was correctly timed, all the cells were damaged, but only the abnormal cells were unable to recover, by virtue of their lower vitality. The application of solid CO_2 was almost painless, and the thawing out of the tissue was disagreeable for a few minutes, but that was all. The great advantage in eye work was the ease and certainty with which the reaction could be circumscribed and controlled as to depth and intensity. Its greatest advantage of all was its efficiency.

ON THE CONDITIONS WHICH MAY ACCOUNT FOR THE GREATER PREVALENCE OF CATARACT IN INDIA.

By Lieutenant-Colonel L. J. PISANI, I.M.S., F.R.C.S.

I.—Distribution of Cataract.

IT has appeared to me that it might be of interest to this Section if I brought before it some statistics regarding the prevalence of cataract in India, and if I incidentally discussed some points in connexion with its geographical distribution. There are no statistics available showing the actual prevalence of cataract, but an approximate estimate can be made from the number of operations performed in the various medical institutions. I have here a statement showing the number of operations performed in all Government and State-aided institutions of British India for the last five years for which complete records are available. You will see at once the great difference in the number of operations. In one of the Northern Provinces, the Punjab, the number of operations averaged 7,636 annually, or a ratio of 308.43 per million of population; on the other hand you have Burma showing only a ratio of 4.74 per million. When the data are mapped out it is seen that cataract is much more prevalent in the northern and central divisions of India than in the lower provinces and in Burma.

The difference between these northern areas and the others is so marked that it cannot be an accidental circumstance. I take for example the three more Northern Provinces, namely, the Punjab, the North West Frontier, and the United Provinces; these with a population of 74,571,999 show a yearly average of 13,481 operations; the remainder of India, with more than double the population—namely, 165,622,924—shows less than half the number of operations—namely, 6,216. Certain hospitals in India have a great reputation as eye hospitals, and some well known operators attract a large number of cases; but my personal experience is that among the class of patients who attend hospitals very few

Number of Cataract Operations Performed in the Government and Aided Institutions of British India during the Five Years 1904 to 1908.

Provincial Administration.	Population.	1904.	1905.	1906.	1907.	1908.	Total for the Five Years.	Average Yearly.	Ratio per million Population.
Punjab	24,754,737	7,164	7,680	8,009	7,577	7,753	38,183	7,636.3	308.43
North-West Frontier Province ...	2,125,480	301	249	308	545	346	1,749	349.4	164.38
United Provinces	47,691,782	5,023	4,994	6,421	5,568	5,474	27,480	5,496 0	115.23
Central Provinces and Berar ...	14,627,045	681	777	828	871	973	4,130	826 0	55.78
Bengal	50,528,446	2,194	2,239	2,714	2,883	3,268	13,304	2,660.4	52.45
Bombay	25,424,235	1,012	917	1,010	962	985	4,886	977.1	38 03
Madras	36,744,483	1,346	1,149	1,232	1,404	1,034	6,165	1,235.0	33.58
Eastern Bengal and Assam ...	29,812,735	62*	347	519	708	749	2,384	476 4	15.89
Burma	8,485,980	24	31	37	56	65	213	42.3	4.74

* Assam only.

go beyond their own province. Such may not be the case in some parts of India, but as more operations are performed in Northern and fewer in Southern India it is unlikely that migration to any great extent can take place, owing to the very great distance. I regret that I have been unable to obtain statistics for the native States of Central India and Rajputana. The statistics dealt with in this paper refer entirely to British India. The conditions in the native States are generally different, and the medical institutions are partly maintained by the Government of India and partly by the durbars of the native States.

II.—*Climatic Conditions.*

It has been stated that cataract may be less prevalent in the rice-eating areas than in those parts of India where wheat and other similar grains is the staple food of the people; but the fact that Mohammedans, who are meat eaters, suffer as much (Dr. Neve of Kashmir, who has investigated the subject, has stated that Mohammedans suffer more) is against this theory, and I think the general opinion of medical officers in India is that diet has no connexion with cataract.

Briefly it may be stated that there are three seasons in India: a hot, a rainy, and a cold season. In Northern India the winter is colder and the hot season hotter than in Southern India, where the variations of temperature are comparatively slight all the year round. The south-west monsoon winds bring rain to India, extending up from the south, but not reaching the extreme north. In addition Burma and North-East India receive rain from the north-east monsoon. One thing I wish particularly to draw attention to is the extreme dry heat which prevails in Upper India from April to June; the dry areas include the North-West Frontier Province and Scinde, the Punjab, the United Provinces, and the immediately adjacent parts of Bengal and the Central Provinces. If you will look at this statement, which shows the meteorological records of various stations in India, you will see how widely the humidity varies. The *lowest* monthly mean percentage humidity, for, example, in Burma is in the neighbourhood of 70; in Eastern Bengal, which is close to Burma, it is about the same; in Madras it is between 55 and 70; whilst in the Punjab, North-West Frontier Provinces, United and Central Provinces, it reaches 30 to 40, or even less. These last-mentioned provinces also record generally the highest maximum temperatures.

III.—*The Probable Connexion between Climatic Conditions and Geographical Distribution.*

Now if we look again at the previous statement of cataract prevalence we find that the areas of greater cataract prevalence correspond with the areas of great dry heat, and that in the areas where the air is saturated with moisture cataract is not common. There is one exception, however—the Central Provinces. I think that the conditions which exist there account for the smaller number of operations; it has a comparatively small population of 14 millions, and there are large areas of wild country where the means of communication are bad and the population is widely scattered.

It is generally believed that in the Himalayas where the heat is never great, cataract is never very prevalent; but statistics of the scattered hill villages are difficult to obtain, as patients frequently wander to the adjacent submontane districts for treatment. I think also that the well-to-do, in proportion, suffer much less from cataract than the ordinary cultivators, who are much more exposed to the heat, working all day, and sometimes living in the fields. This has generally been attributed to deficient nutrition among the poorer classes, but the same conditions of poverty are found all over India without causing a particular prevalence of cataract. There are two factors which may account for the prevalence of cataract in the dry and hot areas, namely: (1) The intense light and glare; (2) the great dry heat.

IV.—*The Influence of Intense Light.*

The light in India is intense, not only directly from the sun, but also reflected from the dry ground, which in the hot season is devoid to a great extent of vegetation. Light has for many years been considered as one of the causes of cataract; even in the eighteenth century writers mentioned it as a probable cause. The condensation of the nucleus and discoloration of the lens, which take place in old age, have been attributed to the continued passage for years of light through the lens. Becker suggested that the ultra-violet rays might have some influence in causing senile changes.

Widmark[1] working with an electric arc lamp of 1,200 candle power at 25 cm. distance, produced lens opacities in 3 out of 12 cases experimented on, and he attributed the opacities to the ultra-violet rays.

Birch-Hirschfeld[2] similarly produced lens opacities with an electric arc lamp, but the ultra-violet rays were removed by means of a glass plate.

Herzog[3] also, with electric light deprived of the ultra-violet rays, experimented on three rabbits. After two hours' action of strong electric light one had total and permanent opacity of the lens; the others after three hours had noticeable opacities, but these subsequently completely disappeared.

Ogneff[4] made a number of experiments on frogs, pigeons, and rabbits, with electric lights of 5,000 to 8,000 candle-power and failed to produce opacities of the lens.

Strebel,[5] Hertel,[6] and Birch-Hirschfeld[7] (using the Finsen or magnesium light) were not successful in producing an opacity with ultra-violet rays even when all the symptoms of intense action of the rays were evident in the anterior part of the eye.

Widmark is the only experimenter who has claimed to have produced opacities with the ultra-violet rays. It is noticeable, however, that he used an electric arc lamp.

Terrin[8] has very carefully recorded his experience in 45 cases occurring among the employees of the Paris Metropolitan Railway. In many of his cases there had been an accidental exposure to intense light from currents up to 550 volts at a very short distance from the eye; and although most of these men suffered intensely, and many permanently, from the effects of the exposure, in not a single case was the injury followed by cataract. He, working with Weiss, also failed to produce opacities in dogs with the electric light.

The experiments of Birch-Hirschfeld with ultra-violet rays are also of particular interest. He produced in animals with these rays electric ophthalmia, electric blindness, and the symptoms of snow blindness, but he utterly failed to produce opacities in the lens.

There are no cases on record of the artificial production of lens opacities by means of sunlight.

V.—*The Influence of Heat.*

Heat may act directly on the eyes, or it may act indirectly by modifying the composition of the aqueous. During the hot season the shade temperature sometimes rises in India to 120° F., and the temperature in the sun's rays to 160° to 165°. The percentage humidity may only be 40°, or even as low as 15°. At times, especially in the early part of the hot weather, westerly winds are very prevalent and are almost unbearable from the high temperatures. As an instance of the great heat, I may mention that I was never able to wear dark glasses owing to their getting so heated as to produce an intensely uncomfortable action of dryness in the eyes. Under these conditions which prevail in Upper India it can be imagined that there must be an intense local action on the eyes and indirectly on the composition of the aqueous by the excessive loss of fluid from the system. However frequently this loss may be replaced, there must be derangements, and these, even if temporary, may, when frequently repeated, modify the composition of the aqueous.

Outside the body the lens albumen coagulates at a temperature of 52° to 62° C. (125.6° to 143.6° F.), but with gradual heating the lens can stand a temperature of 65° C. (149° F.) without becoming cloudy.

It will now be necessary for me to consider some of the theories connecting the formation of cataract with an alteration in the composition of the aqueous. That an alteration of the composition of the aqueous may be the cause of cataract has been stated by many writers. That such a belief should exist is not to be wondered at, as a concentration of the aqueous artificially produced has so frequently been shown to produce opacities in the lens. The alteration in the composition of the aqueous has been believed by some to be due to an increase of albumen, by others to an increase of its saline constituents. Peters,[9] who has written extensively on the subject, believes that there is primarily a senile change in the epithelium of the ciliary body which produces an increase of sodium chloride in the aqueous. Troncoso,[10] who has more recently examined the aqueous in 9 cases of cataract, found an increase of saline constituents, and he believed that cataract was formed by a dehydration of the lens.

In naphthalin cataract both Peters[9] and Manca and Ovio[11] also found an increase of sodium chloride in the aqueous.

Romer[12] believes that cataract is formed by an accumulation of decomposition products in the body, which he names cytotoxins, and which act injuriously on the lens.

Quite recently Scalinci[13] has devolved a new theory based on the chemical properties of the lens. He considers the lens to consist of a mass of "organic hydrogel" consisting of two colloids, which he calls phacoproteine; of these one is soluble and swells out, the other is insoluble. In normal conditions these colloids are found in a state of alkaline proteine. He believes that faint organic acids such as acetic, oxybutyric and formic acids, find their way to the aqueous, and that these by neutralizing the phacoproteine form precipitates in the lens of neutral phacoproteine which is the cause of the opacities. He explains tetanic cataract by the formation of lactic acid, and glassblowers' cataract by an excess of carbonic acid in the aqueous.

In connexion with alteration of the aqueous it may be mentioned that in the last stages of cholera a cataract is sometimes found from dehydration consequent on the large loss of fluid from the system.

VI.—*Experimental Cataract.*

The lens is, as well known, extremely susceptible to changes when exposed outside the body or when immersed in fluid. Opacity of the lens has been experimentally produced in animals by the injection of various substances into the body either subcutaneously or into the anterior chamber.

Many years ago Kunde[14] showed that the lens could be rendered opaque in a frog by the injection into the system of chloride of sodium, nitrate of soda, or even of sugar. By placing the animals subsequently into water the lens became clear.

Magnus produced opacities in young cats five hours after the injection of 10 to 20 grams of salt.

Deutschmann produced an opacity by the injection of salt solution into the anterior chamber, and Hess has also produced it with a 4 per cent. solution.

Kuhnhorn and others have produced an opacity with calcium chloride.

Naphthalin cataract has been frequently produced in rabbits, and the aqueous found to contain an excess of salt.

The opacity of the lens in experimental cataract is generally found in the anterior part of the lens, and is caused by a dehydration of the lens; a similar dehydration occurs if the lens in its capsule is placed outside the body in a solution of sugar or salt.

As regards diabetic cataract, it may be stated that the amount of sugar found in the aqueous in man is very small—in fact, an amount which would not produce an opacity in an experimental case; but there is no doubt that the presence of sugar, however slight, must be the cause of the cataract, either directly by affecting the capsular epithelium, or by interfering with the nutrition of the lens.

VII.—*Cataract in Bottle Makers.*

A particularly interesting condition, which has been attributed to the great heat devolved in the manufacturing process, is the cataract found in bottle makers. In 1886 Meyhoffer[15] first prominently brought to notice that the workers in this industry were particularly subject to cataract, and the condition has since been frequently described. Meyhoffer believed that it was due to the enormous sweating and local abstraction of fluid, especially on the left or more exposed side of the body. Hirschberg[16] believes that in long exposure to high temperature so much heat is absorbed by the lens as to cause fine internal changes and subsequent loss of transparency. Robinson,[17] who published a number of cases in 1903, believes that heat is the cause. According to him, it is the maker of "ordinary bottles," and not the workers on fluid bottles, who suffer most, as they are subjected to a much more intense and prolonged heat. These workers have to look into tanks evolving a temperature of 2,500° F., and to protect them from the glare they look into the tanks through dark glasses. It may be stated that as cataract is not prevalent among other workers exposed to great heat, such as in steel and iron works, some writers (Snell[18]) believe that Robinson's conclusions are incorrect. Robinson,[19] however, explains the non-prevalence of cataract among steel-workers by the period of exposure to intense heat being very short, and not sufficient to cause cataract. This form of cataract has by some been explained as being due to a degenerative change in the vessels due to the hard work and exposure to great heat. The opacity is situated at the posterior part of the lens, in fact, at the nodal point, and has for this reason been believed to be produced by the light rays.

VIII.—*Conclusions.*

As regards light, our present knowledge may be summed up as follows:

1. In those parts of India where light and glare are intense cataract is very prevalent.
2. The position of the opacity in glass-blowers' cataract suggests that light may be the cause of this form of cataract.
3. Cataract has not been artificially produced by sunlight.
4. Cataract has been artificially produced by electric light in animals.
5. Exposure to intense electric light in man does not produce cataract.
6. No reliable case has, up to the present, been recorded of the production of lens opacities by the ultra-violet rays
7. That the inhabitants of mountainous regions, where ultra-violet rays abound, would, if these rays were the cause of cataract, be the greatest sufferers, which is not the case (Birch-Hirschfeld).

As regards heat, we may sum up as follows:

1. That great dry heat prevails in those parts of India where cataract is common.
2. That direct heat has not been proved to produce cataract.
3. That glass-blowers' cataract can be as well explained by a heat as by a light theory, as both conditions coexist.
4. That concentration of the aqueous by the introduction of sodium chloride into the system, or into the anterior chamber, produces lens opacities.
5. The aqueous has been found to contain an excess of sodium chloride in men suffering from cataract.
6. That great loss of fluid from the system in cholera has been known to produce cataract.

There may be more than one form of cataract, each produced by a different cause, as suggested by Hess, but whatever the cause or causes may be, one condition is necessary, the presence of a "senile lens"—that is, a lens undergoing nuclear condensation. It is at the time when accommodation fails that cataract commences. The anterior part of the lens and its capsule are the parts which for years have been subjected to the greatest strain in connexion with accommodation, and this is perhaps the most vulnerable spot during the period when senile changes are taking place in the lens.

The conditions existing in India, which I have already described, suggest that exposure to prolonged intense dry heat may produce an alteration in the composition of the aqueous, either by producing a concentration of its normal constituents or by the addition of something abnormal to its composition, which may injuriously act through the anterior capsule starting the formation of cataract.

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DISCUSSION.

Mr. CHARLES WRAY (London) said the relation of humidity to cataract was in confirmation of his observations that cataract in England was most frequent in subnormal water drinkers.

Major GIDNEY, I.M.S., thought the essential factor in the production of cataract was premature senility, and that climatic influences were of little consequence. He said little reliance could be placed on official statistics of cataract extraction, because they took no account of the enormous number of cataracts that were operated on by quacks.

Mr. ANGUS MACGILLIVRAY (Dundee) said he had a considerable knowledge of Anglo-Indians engaged in the jute trade. He did not find a residence in India increased their liability to cataract.

Mr. CECIL SHAW (Belfast) said that reference should be made to the recent report of the Belgian Ophthalmological Society into the causes of accidental injuries to the eyes by electricity. There was an admirable summary of the influence of extreme light effects, and there appeared a large number of cataract cases.

Colonel PISANI, in reply, said that the jute trade was carried on in the moistest part of India, where cataract was least prevalent.

DISCUSSION ON THE DIAGNOSTIC VALUES OF OPHTHALMOPLEGIA, PARTIAL AND TOTAL.

OPENING PAPER.

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IN opening this discussion it seems to me advisable that I should try to evoke definite expressions of opinion on certain points that are generally accepted as proved, as well as on others on which we may be in some doubt. The way in which statements are apt to be handed down from one textbook to another, without question as to their accuracy, until at last some one sees reason to doubt the truth of what has been for long accepted as a fact, makes it particularly important that the chance of a critical review on an occasion like the present should not be lost. To the neurologist no aspect of the subject set down for discussion to-day is more important than that to which I propose to direct attention at the outset; for no problem that he is asked to attempt to solve so often presents itself as that which requires him to determine whether an affection of the nervous system is organic, or merely functional.

Paralysis of ocular muscles has been regarded as one of the most reliable signs of organic disease, as opposed to a functional affection of the nervous system, and I have never had reason to question the correctness of this view. This is, however, a fitting occasion for the reconsideration of this important question, and for an authoritative pronouncement that may further establish the truth of the view that has been hitherto accepted, or for the chronicling of any facts that may legitimately be regarded as discounting the value of ocular paralysis as a sign of organic disease. The question becomes especially important when considered in connexion with the early manifestations of disordered functions of the nervous system that are to be attributed to disseminated sclerosis, for diplopia and ocular paralysis are among the earliest clinical manifestations that may be the outcome of this grave disease. There is much in the fleeting character of many of its earlier manifestations that suggests the probability that a functional rather than an organic affection obtains. So that it is not possible for us to have too many reliable signs of organic disease on which we can rest assured of our diagnosis. Would paralysis of one or more eye muscles, amidst a variety of subjective symptoms that might as readily be accounted for by functional disturbance rather than organic disease, be an adequate reason for the latter diagnosis, in spite of the absence of any other sign on which we have learnt to place reliance as an indication of organic changes in the nervous system? It is only by placing ourselves in a position of the kind, and by eliciting a definite reply to such a question, that we can be in a position to feel secure of our ground when in any case we allow paralysis of eye muscle to weigh in the favour of the diagnosis of organic disease. For my own part, as I have already indicated, it would be impossible to arrive at any other conclusion than that the affection is organic in the presence of ocular paralysis, no matter how isolated this manifestation might be, in so far as the other organic signs are concerned, and no matter how strong the temptation otherwise to regard the affection as functional.

The question as to whether a disability is due to spasm, as opposed to paralysis, naturally enters into our considerations in this respect, and the need for distinguishing disability due to the one cause as opposed to a defect that is due to the other, is too evident to need emphasis from me. I cannot, however, refrain from referring to an interesting case that bears on this question, in which a woman with other signs of disseminated sclerosis was unable to move the right eye outwards, although the left eye moved inwards to the full extent, on her attempts to look to the right. There seemed nothing remarkable in this, for the reason that paralysis of the external rectus was what might reasonably have been expected in a case of the kind. On further examination of the patient, however, she proved to have, in addition to the signs which pointed

to disseminated sclerosis, a condition of myotonus, such as is characteristically seen in the malady known as Thomsen's disease, so that when she squeezed an object with either hand she was unable to relax her grasp for some seconds. In the light of this information, and remembering that I had tested her power of convergence of the eyes before I attempted to elicit the lateral movements of the globes to the right, I reinvestigated the ocular movements, with the result that the right eye now moved outwards to the full extent, and in a way that was altogether impossible when I first attempted to elicit this movement. There seemed no reasonable doubt that in this case the inability to turn the eye out at first was due to the restraining effect of the internal rectus, which would not relax so as to allow the globe to be moved in the direction in which the external rectus was attempting to displace it.

It is not only with hysteria that we are concerned, however, when we speak of a functional affection of the nervous system. And so many of the organic affections are for a time hidden under the guise of what appears to be neurasthenia, that ocular paralysis affords us one of the means of determining whether neurasthenia alone exists, or whether some organic affection of the nervous system underlies the symptoms which appear to be due to nervous exhaustion. Thus it comes about that two affections like neurasthenia and myasthenia gravis, closely similar, it may be, in so far as the patient's complaints are concerned, but widely different in their nature and prognosis, may be at once distinguished from each other in the presence of ocular paralysis; for while such paralysis is one of the recognized features of the serious disease myasthenia, no such paralysis is to be accounted for by the uncomplicated condition of neurasthenia.

The fact that there is paralysis of eye muscles does not only exclude functional affections, but it also serves to rule out of court many organic disorders of the nervous system, some of which may resemble, in certain of their other manifestations, diseases in which paralysis of eye muscles forms part of the recognized clinical picture. I need only remind you in this connexion of affections like subacute combined degeneration of the spinal cord, and the ordinary varieties of peripheral neuritis, excluding in this connexion the diphtheritic form, in which paralysis of eye muscles forms no part of the clinical picture, as contrasted with diseases like disseminated sclerosis in the one case, and tabes dorsalis in the other, in which such paralysis of ocular muscles is common. In this way the number of diseases to be considered as possible in any given case can be considerably reduced. But although I heartily invite adverse criticism, and welcome proof to the contrary, it is my belief that we cannot go much further in diagnosis from a consideration of the ophthalmoplegia alone, and that to arrive any more closely to a diagnosis than this it is necessary for us to take into consideration at least concomitant ocular phenomena, or even, it may be, evidence that it is only to be secured by an examination directed towards ascertaining the state of the whole of the nervous system, and possibly of the internal viscera as well.

Do not let it be supposed that these remarks are intended to belittle the value or importance of ocular paralysis in diagnosis, for all that is intended is that we should be fair to ophthalmoplegia if we are to expect help from this source in our clinical work, and that we should accordingly consider the ocular paralysis in conjunction with all the evidence that we can collect, rather than attempt to divorce it from everything else that may obtain in the case, and expect it to do the impossible. We are all fully alive to the extreme importance of ocular paralysis in the diagnosis of intracranial tumour; and the patients subject to attacks of severe headache, whether this be accompanied by vomiting or not, cause us the greatest anxiety if ocular paralysis should appear, for intracranial tumour, or some equally grave condition, would naturally be suspected. Nevertheless, the knowledge of that rare but well-recognized affection, migraine ophthalmoplégique, in which recurring ocular paralysis forms part of the clinical picture, affords a possibility of escape from a diagnosis so grave. It would be idle, therefore, to suppose that we do not welcome any other evidence that can be expected to assist us in arriving at a correct conclusion on so important a question, and thus we search for optic neuritis, paralysis of other cranial nerves, or evidence of pyramidal or other tract involvement. I have not been guilty of the error of mistaking as intracranial tumour a case in migraine ophthalmoplégique, but I have to confess to a mistake in the opposite direction in one case, in which recurring headache became complicated by ocular paralysis, which improved and then relapsed in a way that made it seem probable that migraine ophthalmoplégique, rather than intracranial tumour, was to be held responsible for the symptoms. The absence of all other signs that could assist me, and the fact that the patient seemed in no way gravely ill when her headache was not severe, permitted me to fall into an error that would have been averted had there been paralysis of any other cranial nerve, optic neuritis, or one or other of the various other signs on which we depend for the diagnosis of an intracranial tumour.

Not only in the diagnosis, but also in the localization, of intracranial tumours, however, do we look for assistance from ocular paralysis, as is as well illustrated by what obtains in connexion with the cerebellum as by any other example that I can suggest. The indications of intracranial tumour may be clear, and those pointing to the region of the cerebellum equally so, but to cranial nerve paralysis, among other signs, we look for assistance in our attempts to determine whether the tumour be intracerebellar or extracerebellar; and no such paralysis is more common than that of eye muscles—a condition of things which obtains in the former variety but not in the latter.

Mode of Onset and Progress of Paralysis.

At the first glance, here, at least, we would appear to have reliable data on which to base a positive diagnosis; but only a few moments' consideration are necessary to make it evident that fallacies are once more liable to creep in, which can only be avoided by treating the subject in a much more liberal spirit than by attempting to arrive at a diagnosis in this evidence, unsupported by information of a more general character.

It is tempting to assume that a vascular lesion is to be held responsible for a paralysis of sudden onset, and that to a sclerosis can only be assigned such paralysis as is slow in onset and progress. A few moments' consideration will, however, be sufficient to make it obvious that such a position is quite untenable, and, at the risk of wearying you by repetition, I must once more remind you of what so often occurs in disseminated sclerosis. It seems incredible to many who are not conversant with the vagaries of this disease, that a malady that has as its anatomical basis a sclerosis can possibly occasion a paralysis of ocular muscles that is sudden in its onset, and which possibly results in recovery after persisting for a time. Nevertheless, this is what commonly happens; and, although the true pathology of disseminated sclerosis has yet to be determined, there is in this, and in other known facts with regard to the disease, something which justifies the assumption that some other morbid process is at work and interferes with function antecedent to the establishment of the sclerosis, however much difference of opinion there may be as to what the precise nature of that process is.

The Significance of the Combination in which Ocular Muscles are Affected.

At the risk of being regarded as altogether too destructive in my methods and not sufficiently constructive, I am compelled to question how far we can rely on such a combination of paralysis of ocular muscles as is supposed to indicate an intramedullary lesion which involves the nuclei, rather than an affection of the peripheral nerve, that is to be held responsible for the paralysis which is present in any given case. On our being able to be assured which is the seat of the lesion much may depend, not only in regard to diagnosis, but also as to prognosis and the possible effects that are to be expected from treatment. Are we justified in placing much reliance on the combinations of ocular paralysis usually supposed to indicate that the lesion is intramedullary, as opposed to its being peripheral? Or have others who are to take part in this discussion had experience similar to my own, which makes it impossible for me to accept such evidence without a certain amount of hesitation and reserve as apart from many other considerations which must enter into our

deliberations if a correct opinion is to be formed? It has been my experience that a combination of ocular paralysis, supposed to be pathognomonic of a central lesion, has in reality been accounted for by a peripheral affection. This affords me an opportunity of referring to a case the true pathology of which is obscure, and in which, coupled with certain other signs of organic disease of the nervous system, was paralysis of the lateral movements of the eyes, which made it in every degree probable that an intramedullary lesion of the pons must be present to account for this and the other phenomena that obtained in the case. That the inability to move her eyes to the right or left was not due to any want of appreciation of what was required of her was made abundantly evident by the way in which she carried out all other commands during the investigation of her case, including the movements of her eyes in the upward and downward directions. Nevertheless, on necropsy no lesion could be determined in the pons to account for the clinical phenomena. It is true that the paralysis was recovered from before the patient died, but the interval that elapsed between the occurrence of this paralysis and the time of the patient's death was not sufficient to make it possible that any gross lesion in the pons could have so far recovered as to leave no traces of its former existence. The woman suffered from valvular disease of the heart, which proved to be due to malignant endocarditis; but the onset of her symptoms, though somewhat rapid, was not sudden, so that embolism could not reasonably be entertained as the probable cause, although thrombosis was quite possible.

Although pointing to such fallacies as may arise in connexion with ocular paralysis in such combination as would suggest an intramedullary lesion, I do not wish it to be supposed that one would do otherwise than regard as important in diagnosis and localization such combinations as we usually ascribe to intramedullary lesions, although always keeping in mind the possibility of such errors as those that I have tried to indicate as possible.

The Value of Associated Ocular Phenomena as Aids to Correct Diagnosis.

This affords us a suitable opportunity of eliciting information as to how far non-reaction of the pupil to light in a case in which reaction and accommodation is preserved justifies our proceeding with diagnosis. What evidence is there that this phenomenon can obtain in the absence of syphilis? It will, I take it, be admitted that the Argyll Robertson pupil is met with in persons who have had syphilis, and in whom there may or may not be associated paralysis of the external ocular muscles. The important question that arises in this connexion is as to how far such persons are likely to escape diseases like tabes and general paralysis of the insane if with syphilis they have developed this state of the pupils. Here the ophthalmic surgeon must have opportunities of supplying information that is of the greatest possible value in prognosis that those of us engaged in other departments of practice cannot lay claim to.

The frequency with which we are confronted with this sign as part of the clinical picture of two such grave diseases as tabes and general paralysis, must naturally cause us great anxiety when we discover the sign in a person who has had syphilis. The question on which we should like information in this discussion is as to how far persons who have had syphilis and who have developed this sign have been known to escape the progressive diseases that are to be feared. Put in another way, Does this sign mean the beginning of one or other of these progressive degenerative diseases, or is it compatible with a lesion of syphilis which does not carry with it of necessity the tendency to progressive degeneration?

It is impossible for me to leave this part of our subject without inviting discussion on yet another point on which an important issue may hang, on the co-existence of the Argyll Robertson pupil and paralysis of external ocular muscles. It is as to how far this state of the pupils is compatible with the diagnosis of disseminated sclerosis, a disease that is non-specific in origin, as contrasted with diseases like tabes and general paralysis, that are. The undecided manner in which statements are made in some of our textbooks which deal with the differential diagnosis of disseminated sclerosis raises the suspicion in one's mind that the authors are none too certain of their ground, and I confess that, when obliged to express a dogmatic opinion on this subject, I experience no small amount of hesitation in doing so, for the reason that I so often see patients suffering from disseminated sclerosis in whom this phenomenon does not exist, that I can excuse myself for the scepticism which arises in this connexion which makes me wonder whether the exceptional cases in which the phenomenon has appeared to exist are to be ascribed to faulty observation.

Let me add that I welcome in our discussion any evidence that can be adduced pointing to the existence of the Argyll Robertson pupil in other affections of the nervous system in which syphilis plays no known part in their etiology.

Deductions Warranted by Concomitant Affections of Other Cranial Nerves.

Concomitant affection of other cranial nerves would appear at once to remove the case from the category that concerns the ophthalmologist to the exclusion of those engaged in other branches of medical and surgical practice, and it will probably be conceded that this is the case if we make one exception in so far as the ophthalmic division of the fifth nerve is concerned. Moreover, the fact that other cranial nerves are involved in conjunction with those which control the eye muscles, serves to reduce the number of affections that must enter into our deliberations in attempting to arrive at a diagnosis. So that whereas the fact that there is ocular paralysis at once rules out of consideration a number of affections that otherwise would have to be borne in mind, a still larger number of diseases may be excluded when we find added to the ocular paralysis a similar affection of other cranial nerves.

Importance of a General Examination.

Is a diagnosis ever justified from a study of the ophthalmoplegia in conjunction with symptoms without the additional assistance that can be derived from the general physical examination of the patient? The only hesitation which I experience in giving a decided answer in the negative to this question is the diffidence which I feel in doing so in the presence of an audience that must of necessity be composed very largely, if not exclusively, of ophthalmic surgeons. A discussion of the kind on which we have embarked is, however, intended to assist us in determining what use we are justified in making of ocular paralysis in diagnosis. So that, although the considerations that have engaged our attention this morning appear to me to warrant the conclusion at which I have arrived, no one would be more glad to have the contrary proved. Whether or not it is admitted that a general examination of the patient is necessary before any very accurate opinion can be formed of the significance of ocular paralysis, it becomes necessary for me to invite some of those who are to take part in the discussion to record to what extent they have been aided in determining the significance of ophthalmoplegia by such procedures as examination of the blood and the cerebro-spinal fluid, and by the application of the Wassermann test or one of its modifications. What experience, if any, has there been that such tests have proved of service in determining that isolated ocular paralysis has been of syphilitic origin? It is, of course, no uncommon experience that paralysis of a single eye muscle is met with in the absence of any other physical sign in the individual. Syphilis is commonly suspected under such circumstances, and the fact that the patient recovers from the paralysis under antisyphilitic treatment is often accepted as evidence of the correctness of the diagnosis. In reality, however, no such conclusion is necessarily justified, for it is compatible with a non-syphilitic condition that such paralyses should obtain and nevertheless terminate in recovery. Any information that can be derived from such investigations as can be expected to throw light on the nature of the morbid process responsible for the ophthalmoplegia must of necessity engage our attention in a discussion such as that with which we are now occupied; so that I hope that subsequent speakers will supply us with any information that they have on these points, which must of necessity greatly enhance the value of the discussion.

DISCUSSION.

The PRESIDENT said he had always looked upon these ocular paralyses as indications of grave disease, and he was accustomed to warn patients' medical advisers to that effect, yet subsequent events did not always bear out that prognosis, at any rate for many years.

Mr. L. VERNON CARGILL (London) said that he took it for granted that the ophthalmoplegia in question was one in which the reality of the condition had been established, and refraction, concomitancy, and purely functional or hysterical derangements eliminated. Diplopia would be absent in the conjugate paralyses of cortical and subcortical lesions, and replaced by the symptoms of vertigo and false (excessive) projection towards the paralysed side. Diplopia might also be absent in cases of double ophthalmoplegia externa, the patient moving the head instead of the eyes, and sometimes even being unaware of the defect until informed. It often fell to the lot of the oculist to be the first to detect on-coming serious nerve disease by the discovery of pupil derangements in patients who had sought his advice merely for presbyopia or some error of refraction, but in no cases of ophthalmoplegia could reliance for rational and accurate diagnosis be placed upon the ophthalmoplegia alone. The history of the case might give valuable assistance. It might show it to be of congenital origin, or that there had been a sore throat some three or four weeks previously, pointing to diphtheria as the cause. Ophthalmoplegic migraine would be indicated by a lengthy history of recurrent transient attacks of oculo-motor paralysis associated with severe headache, recovery after the attacks becoming gradually less complete. In other cases there might be a history of injury to the skull or eyes; or of some infective disease, as, for instance, syphilis, tubercle, and rheumatism; or of some intoxication as by alcohol, lead, or arsenic.

An acute onset with febrile and cerebral disturbance pointed to polioencephalitis superior; a gradual onset, the paralysis slowly attacking one muscle after another, indicated a chronic nuclear ophthalmoplegia. The pupillary reaction and fundus oculi must be examined, paralysis of other cranial nerves looked for, the condition of the reflexes determined, the state of the vascular system ascertained, the temperature taken, and the urine tested for sugar or albumen. Evidences of acquired or inherited syphilis or of tubercle might give valuable help in diagnosis. Furthermore, special tests might illuminate a diagnosis otherwise shrouded in doubt, such as the Wassermann test for syphilis, von Pirquet's skin reaction for tubercle, the opsonic index, and the examination of the cerebro-spinal fluid for lymphocytosis or tubercle bacilli. Lastly, the effect of treatment (for example, antisyphilitic) might yield additional corroborative evidence or elucidate an otherwise incomprehensible case.

Amongst the most difficult cases to diagnose were those associated with myasthenia gravis, in which ptosis or diplopia might be amongst the earliest symptoms, and, if found without discoverable origin, require prolonged observation before this disease could be excluded.

The presence of ophthalmoplegia proved the existence of an organic lesion, although hysteria might complicate the case and its symptoms be the most obtrusive.

In uncomplicated hysteria there was no single muscle paralysis, no reflex iridoplegia, no nystagmus, no morbid condition of either fundus oculi, and no typical hemianopsia. Hysteria being due to a functional derangement of the higher cerebral centres, it simulated the ocular paralyses associated with lesions of those centres. Thus, conjugate paralysis was simulated and also ptosis, but they could be distinguished from true paralysis by their "dissociated" character, the direct and voluntary movements being affected, but not the indirect and involuntary. Hence, with inability to look to the right side, the eyes might be made to continue fixing a stationary object, whilst the examiner slowly rotated the head to the left. The ptosis was apparent or "false," and due to spasm of the orbicularis. This was evident by the resistance offered to the finger on attempting to raise the eyelid, and, in unilateral cases, by the eyebrow on the affected side being lower, instead of higher, than the other. In double hysterical ptosis, if the object was fixed with the head thrown back, the eyes might be made to continue fixing whilst the examiner slowly rotated the head forwards, so that the patient was finally looking from under the eyebrows with the upper lids well elevated. In hysterical cases attempts at fixation might lead to spasm of the muscles associated with convergence; blowing on an anaesthetic eye would cause reflex winking in both, and there was disagreement between the visual acuity and the confines of the visual fields for white and colours.

In the ocular examination alone valuable evidence as to the cause or nature of the ophthalmoplegia might be found. The cornea might show evidence of interstitial keratitis or phlyctenular mischief, indicating syphilitic or tuberculous infection. The reaction of the pupils might be imperfect, or almost absent, in consequence of posterior synechiae resulting from past iritis. If there had been only one attack, the iritis might have been syphilitic; if repeated attacks, probably rheumatic. In the absence of any adhesions, the presence of pupillary irregularity, inequality, or abnormal smallness might be suggestive of tabes or general paralysis, and absence of the light and sensory reflexes or of the convergence reaction were more than suggestive. Reflex iridoplegia, or Argyll Robertson pupils, it was now generally conceded, were almost certain indications of antecedent syphilitic infection, either acquired or inherited, and they were often a danger signal of tabes or general paralysis. Argyll Robertson, or fixed pupils, with or without ocular paralysis, might be met with in focal cerebral lesions, especially in tumours of the quadrigeminal region, but general symptoms of intracranial trouble would be present, whilst tabetic signs would be absent. In the absence of patellar reflexes the existence of external ophthalmoplegia together with Argyll Robertson pupils was sufficient to establish a diagnosis of tabes.

Bilateral ophthalmoplegia interna might be the temporary result of mydriatics or ptomaine poisoning. Similarly unilateral ophthalmoplegia interna might be the temporary result of local trauma, mydriatics, or third nerve paralysis; but if permanent, it might also almost certainly be taken as indicative of previous syphilis.

Bilateral cycloplegia, without iridoplegia and without knee-jerks, and with a history of sore throat a few weeks previously, was sufficient for the diagnosis of post-diphtheritic paralysis. These cases nearly all occurred in children, and the diphtheria might have affected the conjunctiva or vulva instead of the throat. In some post-diphtheritic cases the extraocular muscles (the sixth and third in order of frequency) were affected instead of the ciliary muscles. The history, youth of the patient, absence of knee-jerks, normal pupils, rapid onset, transient variation, and quick recovery, served to establish the diagnosis. The choroid might show active or "obsolescent" tubercle, or old syphilitic mischief, or retinal arterio-sclerosis, or diabetic or renal retinitis. These findings, in particular cases, indicating a tuberculous, syphilitic, diabetic, or vascular cause for a coincident paralysis. In pertussis, cases of paralysis of the sixth and seventh nerves, and also of complete ophthalmoplegia, had been reported; they were probably due to small venous haemorrhages affecting the nuclei. It was not improbable that arterial thrombosis or haemorrhage might also affect the nerve nuclei in cases of high arterial tension, with sclerosis, in adults, and the speaker had seen two or three cases which seemed only explicable in this way.

The presence of papillitis or primary optic nerve atrophy might shed a flood of light on an accompanying ophthalmoplegia. Papillitis with ocular paralysis suggested intracranial tumour or inflammation, and in the former case the side on which the papillitis was first developed or most intense would greatly assist the localization. Papillitis, with paralytic internal squint and optic neuritis, due to basic meningitis, had been reported ten weeks after the primary lesion. Primary optic nerve atrophy with ophthalmoplegia might occur in tabes, general paralysis, or disseminated sclerosis, or in lesions about the chiasma or cavernous sinus. In tabes and general paralysis the pupillary phenomena already alluded to, the state of the knee-jerks, and the presence of paraesthesia, would confirm the diagnosis. In disseminated sclerosis the atrophy might be only partial, and nystagmus, exaggerated knee-jerks, ankle clonus, intention tremor, limb weakness, and spasticity would help, with the paralysis perhaps of an external rectus, to complete the clinical picture. Homonymous hemianopsia might be asociated with oph-

thalmoplegia in lesions behind the chiasma. Hemianopic pupillary inaction would not be present if the lesion were supranuclear, but might be detected with infranuclear lesions. Ocular paralysis from lesions in the vicinity of the chiasma would be associated with temporal hemianopsia, and in lesions of the cavernous sinus unilateral blindness would accompany complete unilateral ophthalmoplegia.

Orbital symptoms might indicate that the ocular paralysis had an orbital origin. There might be local oedema, inflammation, and tenderness, unilateral optic neuritis or atrophy, and proptosis or other ocular displacement; and ophthalmoplegia might accompany the exophthalmos of Graves's disease. The extraocular muscles might be damaged by an injury, and a blow on the eye might cause ophthalmoplegia interna, probably from an indirect traumatic lesion of motor ciliary nerve fibres. Some cases of isolated ocular paralysis (for example, the external rectus) with no other signs and an evident rheumatic "diathesis" might be due to neuritis and compared to "Bell's palsy." The speaker had notes of two cases in which paralysis of the superior oblique was accompanied by marked tenderness over the "pulley" on the affected side, suggesting a fibro-synovitis. There was no frontal sinusitis, and no other evidences than definite rheumatism. Might not a rheumatic myositis or fibrositis also pick out an extraocular muscle and be indicated by local pain on movement and tenderness or even slight oedema over its insertion? Paralysis of the third nerve sometimes occurred as a complication of herpes zoster ophthalmicus, and would therefore be accompanied by the obvious local evidences of the latter. The first division of the fifth nerve would also probably be affected, and recovery take place in the course of a few months.

The peculiar characters of the ophthalmoplegia itself might aid in the diagnosis. For instance, in congenital cases there was no contracture of the antagonistic muscles and in some rare instances there had been reported accompanying rhythmical or cyclical changes in the size of the pupils. In myasthenia gravis there might be a variability in the degree of lateral movement at various times, and a fatigue reaction of the pupil had been reported, in which with successive illuminations the contractions became weaker. Isolated, variable, temporary paralysis was frequently met with in the early parasyphilitic cases; whilst in later stages the paralyses were combined and permanent. Conjugate paralyses were the characteristic of cortical or subcortical and of pontine lesions. In the former there was paralysis to the contralateral side, and the latter to the unilateral side.

Dr. E. Farquhar Buzzard (London) said the value of the Argyll Robertson pupil in the diagnosis of syphilitic and parasyphilitic diseases could hardly be over-estimated, although it was somewhat curious that a satisfactory anatomical explanation of so common a clinical phenomenon had yet to be discovered. The point he wished to emphasize was that absence of the light reflex, unilateral or bilateral, as a solitary ocular symptom might be a most important physical sign, not of syphilis, or of parasyphilis, but of the locality of a cerebral lesion. This fact was impressed on his mind about ten years ago by the observation of two interesting cases, and he had constantly found it of use in diagnosis ever since that time.

A woman was admitted into the National Hospital with a history of paroxysmal headache of great severity, having a duration of only a few weeks. The headache had been associated with vomiting, and examination showed a very slight degree of optic neuritis in each fundus. The plantar response was of the extensor type on both sides, probably due to the increased intracranial tension, but careful examination revealed no further physical signs except in the eyes. The right pupil was larger than the left, and did not react to light; the left pupil reacted sluggishly; all other oculo-motor functions were performed in a perfectly natural manner. The patient died suddenly within a few hours, and the only abnormality found *post mortem* was the presence of a cyst, the size of a hazel nut, lying in the third ventricle, attached to the choroid plexus, adherent to the fornix, and blocking the foramen of Munro.

Within a short time of the death of this patient another woman was admitted into the hospital with a history of epileptic attacks extending over three years, and of headache and vomiting during ten months.

She had optic neuritis and slight left hemiparesis, but it was also noted that both pupils were completely inactive to light, although there was no other form of ophthalmoplegia. This patient died, and at the autopsy there was found, not only, as was expected, a large tumour of the right frontal lobe, but also a small cyst, filled with jelly-like material, lying in the third ventricle, attached to the choroid plexus, and impinging on the superior colliculus and the posterior commissure.

As a result of these observations he had formed the opinion that absence of the light reflex was a valuable sign of gross disease in the third ventricle or in the structures immediately surrounding it, and that it not infrequently constituted the first localizing evidence of that disease. He had already mentioned two cases of cysts within the ventricle but had also observed the same phenomenon in the early stages of tumours involving the optic thalamus. When a tumour originated in this situation and tended to grow backwards towards the corpora quadrigemina, it was usual to find the loss of light reflex quickly followed by paralysis of the vertical movements of the eyeballs, the upward being generally lost before the downward movement. The invariability of this sequence made it certain that these two oculo-motor functions had their anatomical seat in the most anterior part of the oculo-motor nucleus and that their impairment could often be detected before there was any other form of ophthalmoplegia. The combination of these signs was sometimes a clue to the diagnosis of vascular lesions of the mesencephalon, and in one instance within his experience proved of value in determining the presence of an abscess in the optic thalamus secondary to suppuration in the thoracic cavity. Recognition of this clinical point might be of practical value, especially in cases of intracranial tumour. In the first place it must be remembered that the pupillary light reflex was rarely lost as the result of papillitis, in fact it was only impaired when optic neuritis was passing on into optic atrophy. For this reason it was important that the loss of light reflex in any individual case should not be ascribed to the presence of papillitis even when there was an extreme degree of swelling, and that some other explanation of the defect should be looked for. In the second place the absence of the light reflex, when not accounted for by optic atrophy, was a contra-indication to any attempt to remove a tumour because it nearly always signified that the latter was too deep for eradication. If, for example, in two cases A and B both presenting vomiting, headache, optic neuritis, and some degree of hemiplegia, case A showed loss of light reflex, that fact must be regarded as an indication that the growth had either originated in the neighbourhood of the third ventricle or had reached that situation and that removal was out of the question. But in case B it would be legitimate to infer the possibility of the hemiplegia being produced by pressure on the internal capsule from without, and to undertake an exploratory operation with some degree of hope that the growth might be accessible. The question of decompression must, of course, be decided on other considerations. The speaker had found this practical point of use both in forming a decision as to the advisability of operative interference and in giving a reliable prognosis, on more than one occasion.

Dr. W. B. Warrington (Liverpool) commented upon the occurrence of apparently simple cases of single ocular muscle paralyses without indications of grave general disease.

Mr. N. Bishop Harman (London) said he believed that there were many cases of single muscle paralyses which were due to exposure to unusual extremes of cold. He had heard no reference to them in the able papers of the neurologists. Mr. Cargill indicated that he recognized rheumatic cases. He could only suppose that these cases did not come into the hands of the neurologist, or else were they to conclude from such a warning that Dr. Russell had given that these were all precursors of grave disease such as disseminated sclerosis? Mr. Harman instanced the case of a man who had been exposed throughout a whole day to a cold east wind on a motor boat, so that he felt ill. The succeeding day he awake with diplopia due to paresis of one external rectus. There were no other symptoms, and good evidence against syphilis. Was it not more likely that such a case was a genuine peripheral neuritis, similar to Bell's paralysis, rather than a precursor of grave disease? He concluded that these cases did occur, and fairly frequently. Acting upon this view, he discarded anti-

syphilitic measures, and prescribed strychnine. In the cases instanced cure was established in three weeks. To view such cases gravely and to give serious warning to the patient or his medical attendant was to court trouble.

The President said he doubted if these cases ever got to a neurologist; to him the diagnosis was established in that they got well.

Dr. R. A. Reeve (Toronto) concurred in the influence of exposure to cold causing isolated paralysis; often he found evidence of deep-seated pain.

Miss Amy Sheppard (London) said she had seen similar cases and obtained evidence of tenderness in the region of the affected muscle.

Mr. A. W. Ormond (London) questioned whether the cases really got well; was the relief only temporary? And, if exposure could be credited with the occurrence, why were these cases not more frequent?

Mr. Angus MacGillivray (Dundee) said he agreed with Mr. Harman in the existence of the cases. But he found that six weeks was the usual duration. He gave a guarded prognosis until the six weeks was up. If at that time the paresis had not passed, he considered the prognosis was grave and indicative of ultimate central disease. He had not found local pain in the temporary cases, but he was sure syphilis was not a necessary factor; further, some of these cases he had watched for twenty years or more, and there had been no recurrence and no subsequent grave effects.

Mr. Hill Griffith (Manchester) asked if Dr. Russell made use of electrical reactions as an aid to diagnosis. He had seen several cases, and had had them under observation for many years, where monocular mydriasis occurred without other symptoms. To what lesion could such uncomplicated stationary paralysis be due?

Dr. John Hern (Darlington) said he had seen many cases of paresis of an external rectus of one eye, such as Mr. Harman had described, and had watched them for twenty years afterwards, and they had not recurred. It was apparently a peripheral lesion, and should be treated as such, when there were no other manifestations to indicate a central lesion.

Major Gidney, I.M.S., said these cases should present no difficulty if syphilis was excluded by the Wassermann reaction.

Dr. Risien Russell, in reply, said he was sorry more definite information had not been forthcoming as to whether failure of a pupil to react to light, while it preserved its action in accommodation, could be relied on as a sign of syphilis. All would agree that the information supplied by Dr. Farquhar Buzzard in regard to the value of loss of the light reflex as a localizing sign in the case of tumours in the region of the third ventricle, was of very great importance. He was familiar with the cases of monocular paralysis of the pupil and of accommodation, but could offer no explanation for them, and suggested that all cases of the kind should be recorded. In what he had said, he did not, of course, mean to suggest that paralysis of one of the oculo-motor nerves could not result from some simple cause, such as neuritis, for he was quite familiar with the class of case in which the condition was notably due to exposure to cold. He was glad to hear a time limit suggested in which recovery might be expected in this class of case, for he had known of instances in which such paralysis had been one of the earliest indications of what ultimately proved to be sarcoma at the base of the skull. Deep-seated pain in the orbit certainly could not be claimed in support of the diagnosis of neuritis where sarcoma was the alternative diagnosis. Even when comparatively rapid recovery resulted, he could not look on oculo-motor paralysis quite so lightly as seemed possible to one of the speakers. The ophthalmologist saw the paralysis of eye muscle, and watched its recovery, but it was reserved to the physician to see the same patient, it might be many years later, with the unmistakable signs of disseminated sclerosis. It must be very rare to have such paralysis brought about by neuritis due to alcohol, for even in the most severe cases of alcoholic neuritis of the limbs the oculo motor nerves escaped. It did, however, very rarely happen that the nerves supplying the eye muscles were so affected. He had not found electricity of any use in the diagnosis of these cases of oculo-motor paralysis.

THE EDUCATION OF HIGH MYOPES.

By N. Bishop Harman, M.A., M.B.Cantab., F.R.C.S.Eng.,
Assistant Ophthalmic Surgeon, West London Hospital; Oculist, London County Council Blind Schools.

The educational treatment of children who suffer from a high degree of myopia, or of a moderate degree when there is evidence of a rapid progress of the disease, is one of considerable difficulty. Until now we have had to be content with mere negative propositions. The ophthalmic surgeon was wont to tell the parents and teachers that the child must not do this or that kind of work, or in the worst cases the parents were told that the child must not go to school.

Negative propositions are always difficult guides of life. It is all very well to be told "Thou shalt not," but it is infinitely better for the seeker after truth or guidance if he can be helped to know what thing he should do. A definite proposition is one that can be fairly tackled; there is a clear aim in view which is an incentive to endeavour whether or no attainment be reached.

So, in the case of myopic children, parents and teachers are scarcely helped by our "must nots." It is no kindness to the child to cut it off from school life, for though we may look with no kindly eye on our own school curriculum, and remember only too well the burden of the cast iron Latinism which marked the average public school, we ought not fail to recognize that school life and the school curriculum has made immense progress in the last dozen years, and particularly is this true of the elementary schools. We may think in our ignorance that in freeing the child from school we are doing him a good turn. This, in my experience of modern schools, which is fairly extensive, is not true; school life to-day is a pleasure to the child, whilst the loss to him of future good which a decent training can give is incalculable. The negative proposition of "no school" is therefore not good practice.

Next in order for criticism comes the direction "no reading." This direction to parents or guardians is all very well, they may try to carry out the injunction faithfully. But it is quite another matter for the child; an injunction to a parent, and conveyed, let us say daily, to the child, does not necessarily bind him. If he can read, he will read; the difficulty of distant vision, and the degree of loss of free action in play which this entails, tend to direct his attention to pleasures that can be enjoyed alone; and of these reading is the chief, so read he will, though it be in secret, and he will acquire habits of poring over books in dimly-lighted, out-of-the-way corners, where he may escape the attention of his guardians. The second negative injunction of "no reading" is, then, nullified.

These two negative propositions are fair samples of how we do not advantageously control the life of the myopic child.

In the next few paragraphs I will describe an attempt which we are now making in the London County Council schools to solve this difficult problem. Formerly myopic children who were inhibited by their medical advisers from attending ordinary schools were drafted into the blind schools, where the official regulations of the Board of Education required that they should be taught the full curriculum of the blind, including the learning of Braille type. The absurdity of this course must be apparent to every one. In the first place, the children were taught at great labour reading and writing in a script which they were never likely to use once they were free of school. Secondly, because at every opportunity the children were found to bow down their heads and read the embossed type with their eyes, with the result that they learned the bad habit of poring over objects under the very worst conditions. Thirdly, there was a particularly bad effect of this training in a blind school which arose as soon as the Employers' Liability Act came into full force. Employers

are wont to ask prospective employees at what school they have been trained, and mention of a blind school effectively spoiled the chances of these youths at the outset of their careers.

For the past two years we have been experimenting with a new educational scheme for these children. "Myope classes" have been instituted. Into these are drafted such children as are too short-sighted to be educated in ordinary elementary schools and yet are too well sighted for blind schools. For the most part these children are myopes; but there is a sprinkling of children with damaged eyes from interstitial keratitis, phlyctenular keratitis, ophthalmia neonatorum, or purulent conjunctivitis of later years, and some with defective vision from congenital cataract. In all cases where vision can be improved by the use of glasses, glasses are worn; myopes wear a full correction or the fullest their medical advisers

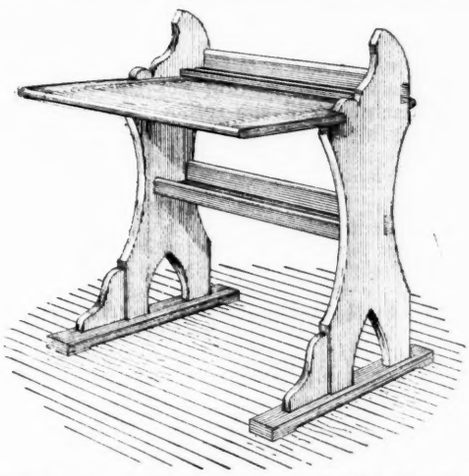

Fig. 1.—Harman's myope desk, in use as table for handicraft.

will allow. The point of view that guides selection of children for this new mode of training is the future prospects of their vision—Will they or will they not be likely ultimately to become blind or nearly blind? If there is a possibility of ultimate blindness they are entered for the blind school; if there is a prospect that they will retain useful vision in later life they are entered for the myope class.

The curriculum of the myope class is arranged under three heads—oral teaching, handicraft, literary work. So much as is possible of mental training is given orally in the ordinary school with their normally sighted mates. The myopic children listen to lessons in history, geography, and nature studies, join in the drill, dancing, and so forth, with the normal-sighted. But the use of class books is forbidden; they are trained to listen and think of what is taught.

The rest of the training is undertaken in their own special class-rooms, which for present convenience and on account of staffing are in a blind school, but in the future and with the growth of these classes they may become independent units.

For the teaching in these special classes two appliances are necessary—tables for handicraft and blackboards for literary work. At first attempts were made to use the ordinary desk furniture of the school, supplemented by a supply of blackboards affixed all round the available wall space, but this was scarcely satisfactory. Now a desk designed specially for this purpose by myself is coming into use. One of these, by the courtesy of Messrs. Hammer, of London, W.C., I show you to-day. This new desk provides the two requirements in one piece; there is a good-sized table for handiwork, which when it is turned up away from the child becomes a large blackboard.

The description of the class furniture gives the clue to the kind of work done in the class. Every sort of educational handiwork, anything upon which a lesson of thoughtfulness and order can be hung, any device of an instructional nature the teachers can bring into usefulness, is taught. Modelling, matmaking, beadwork, simple carpentry, every kind of work which requires a minimum of inspection and a maximum of feeling, is used.

The literary work comes last. In these days some knowledge of the ordinary means of communication is essential to every one; to this end we teach reading and writing and such ciphering as needs script, but as little as possible is taught, for we do not wish to cultivate the desire for literature, and this little is taught in such a manner as will, we believe, inculcate habits of care which may become second nature to the child.

All the reading and writing is done with the use of the blackboards. Copies are set by the teacher in bold thick-lined letters, or with older children subjects are dictated. The child writes free-arm fashion in large characters, each upon his own blackboard.

It is evident that such a scheme of education necessitates a high degree of intelligence—or may I say of common sense?—on the part of the teachers, and, indeed, the teachers have to be taught first. But in our experience the teachers

Fig. 2.—Harman's myope desk, in use as blackboard for litera

have taken up the new scheme of work with thoughtfulness and enthusiasm.

Some of our requirements for these classes are not yet supplied. We want a library of large printed books, or rather scrolls, such as can be hung up in the sight of all the children of a class, and printed in letters of such a size that a child with, say, $\frac{6}{24}$ vision can read with ease at the distance of 2 or 3 metres.

These classes are at present in an experimental stage, but they bid fair to fulfil their intention, and we hope it will not be long before several other similar schools are in working order, for the waiting list of candidates is large

THE OPERATIVE TREATMENT OF HIGH MYOPIA.*

By A. Hugh Thompson, M.D., B.C.Cantab.,
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[Abstract.]

Degree of Myopia suitable for Operation.—The average change of refraction after removal of the lens for high myopia may be represented by the formula $\frac{M}{2}+10.5$, where M represents the degree of myopia before operation. The number 10.5 is an average derived from 22 cases, and in any individual case may be as low as 9 or as high as 13. The degree of myopia which is most likely to be changed into emmetropia is therefore −21 D. The divergence from emmetropia, however, which may be expected to result from needling a myope of, say, −16 D. is not the difference between 16 and 21 or +5 D., but only half this, or +2.5 D. On the whole, we may regard −16 D. as a reasonable limit, higher than which it is justifiable to operate in young patients in cases otherwise suitable.

Age.—Any age up to 25 is suitable where the degree of myopia and other conditions favour an operation. In

* The full text of this paper will appear in the *Ophthalmic Review* for November.

older patients there are more likely to be difficulties from (1) retinal and choroidal complications and (2) increasing hardness of the lens nucleus, rendering absorption after needling difficult.

Growth in the Long Axis of the Eyeball after Operation.—In seven of my cases I have been able to make a comparison between the rate of growth in the long axis of the eye operated on and that in the long axis of the fellow eye. On the average, the tendency, if it exists, does not appear to be affected one way or the other, though its effect on the refraction expressed in dioptres is only about half in the case of the aphakic eye to what it is in the intact one.

State of the Fundus.—Choroidal atrophy beyond the ordinary crescent, macular degeneration or vitreous opacities are all contraindications to an operation. On the other hand, when we operate on an eye free from any of these changes, the chance of their occurring in the future is not affected one way or the other.

Condition of the Second Eye.—The most suitable cases for operation are those in which the second eye is myopic to a less extent than the eye to be operated on. When it is emmetropic there is nothing to be gained by operating on the myopic eye. When, on the other hand, the second eye is seriously diseased, the risk of both eyes behaving in the same way is so great that operation is contraindicated.

Operative Procedures.—There are three possible methods of operating: (1) By repeated small needlings. This method is tedious, as the number of needlings may have to be considerable. (2) By one free needling, followed in from three to ten days by artificial evacuation of the lens matter, and in many cases by a final needling of the posterior capsule. This is the method which I have adopted in most of my cases. In 5 out of 20 eyes operated on I had to repeat the primary needling. In 4 I have been able to dispense with artificial evacuation, but in 3 others that procedure had to be repeated. In 2 cases there have been adhesion of the lens matter to the corneal scar, and these have required division. In 8 cases a posterior capsule had to be divided before the patient saw well, and in 4 others, after remaining transparent for a year or more, the posterior capsule or a membrane in the position of one had to be divided at a later period. Altogether 65 operations were performed on 20 eyes, or an average of 3·25 per eye. (3) The third method is linear extraction of the lens without a needling at all. The objection to this is the danger of vitreous prolapse. I have only performed this in one case, where, the lens being dislocated, efficient needling was impracticable.

Visual Results.—Of 20 eyes operated on, distant vision was improved in 17 cases, and in 10 cases it was better without any glass than what it had previously been with a correcting glass. Of the remaining 3, one was a failure owing to a thick cyclitic membrane, a second was a complete success for five years, but then developed cyclitis with "K.P." and diminished field—probably a case of detached retina—while the third had a macular haemorrhage eight months after operation, after which, however, the eye so far recovered as to see $\frac{1}{18}$. With regard to near vision, in 5 cases the operated eye was used for all purposes with or without a glass, while in 8 the patient preferred to use the unoperated eye for near vision.

Operating on the Second Eye.—It is doubtful if operation on the second eye should ever be done; in any case it should not take place until the lapse of two years after the first operation. If the first eye has done well, the further improvement to be gained can only be relatively slight. If it has done badly, it is *prima facie* evidence that the risk attending operation on the second eye is greater than usual.

Possible Causes of Failure.—Failure may occur (1) as the direct result of the operation, owing to either infection, secondary glaucoma or cyclitis; (2) from causes unconnected with the operation—for example, macular degeneration, choroiditis, or haemorrhage; (3) from detachment of the retina. Some of the cases of detachment would have occurred quite independently of the operation; others probably would not. Previously published cases show a great preponderance of detachments occurring in the first year after operation, and for this preponderance the operation may be held responsible. In a large proportion of cases with detached retina the presence of vitreous opacities before operation has been noted. In a further large proportion a loss of vitreous at the operation has been recorded. These facts ought to make us careful to choose only cases with healthy fundi for operation, and in our operative procedures to take every precaution against a loss of vitreous.

DISCUSSION.

Mr. CHARLES WRAY (London) said he found that recent statistics confirmed results he published some years ago, that the removal of the lens did not stop the progress of the myopia. He thought the operation seriously increased the risks of blindness, and cited three cases of young subjects whom he had recently seen quite blind.

Mr. HILL GRIFFITH (Manchester) said he had operated upon 40 to 50 cases, and in no case had the eye been lost. He did free needling followed by suction. He never attempted the operation unless No. 1 Jaeger test type could be deciphered. He did not think the operation induced detachment of the retina unless acute cyclitis followed, and this might occur in any extraction, infantile or senile.

Major GIDNEY, I.M.S., said his experience of the operation was satisfactory.

A PLEA FOR THE MORE EXTENDED USE OF BUCCAL MUCOUS MEMBRANE IN ENLARGING CERTAIN FORMS OF CONTRACTED SOCKET.

By ARTHUR H. BENSON, F.R.C.S.I.,
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THERE are few more unsatisfactory jobs that one can be asked to perform than the enlargement of a contracted socket, so as to enable it to hold an artificial eye, and so improve the personal appearance of the wearer.

Numerous, and at times terrific, have been the attempts made and advocated for the purpose, and in some instances the results, though surgical triumphs, have proved cosmetic failures.

I need not detail the heroic methods of Grünert, or the more moderate ones of May and Hotz, Weeks, Gullstrand, and the rest, nor yet the ingenious and admirable operation devised by my friend and colleague, Dr. Maxwell. I only wish to enter a plea for the more extended use of mucous membrane in place of skin or epithelial flaps in the plastic operations on the contracted socket.

Of course I am aware that the particular form of operation that suits one case will not be at all suitable to the next, and that each patient must be treated according to the individual requirements of that particular case. And, further, that this fact accounts for the large variety of operations advocated.

A method of operating which I have found useful in a good many cases—modifying it in each instance to suit the requirements of the individual—is something as follows: The floor of the shallow socket is split longitudinally from canthus to canthus. The conjunctiva is dissected down as far as possible towards the lower lid border, and by incision or excision any fibrous bands that are found are removed, and the socket deepened as required. The free margin of the conjunctiva that has been dissected down is now sutured in such a way as to cover as much of the raw surface of the lid as possible. The sutures, with two needles, are passed through the thickness of the lid from within, and are brought out on the superficial (skin) surface, and tied over glass beads. The same thing, if required, is done to the upper lid. It will then be seen what amount of raw surface remains to be covered with mucous membrane.

A large sized Snellen's or Knapp's clamp is next applied to the lip, and a piece of mucous membrane of the required size and shape dissected off. This is sutured in four or five places to the raw edge of the conjunctiva, and the socket is packed with small pellets (the size of a pea) of

cotton wadding impregnated with xeroform. These assist very materially in keeping the transplanted flap in close apposition to the raw surface which it is intended to cover.

The whole socket must be firmly packed with these pellets in every crevice, so that the lids are stretched over the packing. A bandage is used to keep the lids firmly closed. In two or three days the transplanted membrane has become revitalized and adherent in its new bed. The dressing pellets may now be removed and the flap inspected. If all is well, a leaden plate, cut and moulded to suit the requirements of the case, is introduced, and the lids again closed and bandaged. The plate should be as large as possible. A larger plate can often be put in the next day or two, and the glass eye can be used in a week or ten days. If it is preferred, a glass shell can be used from the first, but I prefer the leaden plate.

Various modifications of this procedure may be required. In some cases it is better to leave an island of conjunctiva in the centre of the socket, and make two separate encircling incisions before dissecting up the conjunctiva. In this case, of course, two flaps of mucous membrane must be transplanted. If there is difficulty in obtaining a flap of mucous membrane large enough to cover the raw surface, flaps may be sutured together and applied as one. Again, a deep incision—almost to the external skin—may be made below, and a mucous flap transplanted, and sutured to its lips. A pocket, lined by mucous membrane, is made by passing two or three sutures (with two needles to each suture) from within, and tying them on the skin surface over beads or tubing. Or a well-cut leaden plate may be inserted into the pocket and the lids closed and bandaged. Or the sutures and the lead plate may both be used together. This obtains a mucous-lined pocket something like the skin-lined pocket aimed at by Maxwell, but it avoids altogether any external incision, and any disfigurement from scars, or from the loss of skin. The lids may, if necessary, be sutured together, but this is not always needed. Again, Doyne's sutures may, later on, be employed with advantage in some cases.

Mucous membrane has many advantages over other coverings for a socket. It is much less bulky than skin. It can, without injury, be put on the stretch and dragged into the required shape in a way that skin or a Thiersch flap would not stand. It is very easily manipulated, and is possessed of great vitality, so that sloughing of any part of it is by no means common. It is capable of very rapid revitalization, and so seems to be little provocative of cicatricial contraction under it. Being mucous membrane it becomes at once accustomed to its new surroundings, and having been used to the pressure of the teeth it feels little resentment when a leaden plate or a glass eye is placed in contact with it.

It is, of course, essential that any piece of transplanted membrane should be kept in close apposition to the raw surface to which it has been transplanted, therefore the careful packing of the socket with the pellets is most important. There is no necessity for any elaborate arrangement of warm dishes to hold the mucous membrane when dissected off the lip. The heat of the operator's hand is quite sufficient. Even in winter I usually just lay it on my left hand while I am snipping off all the submucous fat, etc.

If two flaps are required, it is a good plan to partially dissect up *both*, before completely separating *either*, from the lip. This shortens the time of total separation; but, as a matter of fact, buccal mucous membrane will stand a lot more pulling about than skin will. Mucous membrane does not shrink in anything like the same degree that skin does, and also, being thinner, much smaller flaps suffice.

There is not the same tendency to late contraction and interstitial absorption as is found in skin. I should apologize for bringing forward such an old and tiresome subject, but my object is to draw attention to the value of buccal mucous membrane in plastic operations on the eyelids and socket. What I have said regarding its use in the socket is equally true for symblepharon.

In Ireland we see a large number of sockets contracted by reason of the shrinkage following granular ophthalmia, and it is on such that I have found much advantage from mucous membrane transplantation.

DISCUSSION ON THE MORE CHRONIC FORMS OF ANTERIOR UVEITIS.

OPENING PAPERS.

I.—W. T. Holmes Spicer, F R C.S.,

Ophthalmic Surgeon, St. Bartholomew's Hospital.

[Abstract.]

Mr. Holmes Spicer, dealing with the extraocular manifestations, remarked that it was curious that the same subject had been dealt with by Professor Fuchs of Vienna at the last London meeting of the Association. He would exclude the evanescent type of episcleritis described by Fuchs, and also pustules beginning in the conjunctiva which were local affections, also gumma of the ciliary region spreading externally. That left him scleritis and episcleritis. Textbooks distinguish between episcleritis and scleritis. There was no essential difference between them, save the variation in incidence with the time of life and the severity of the inflammation.

So far as their connexion with uveitis was concerned, that depended upon the depth and intensity of the inflammation. He found two types of scleral nodules—one hard and raised, which on incision proved to be mostly solid, though occasionally a bead of pus was found within. The second type presented a boggy, undermined condition; the surface was blue, soft, and yielding. Some cases began as a vesicular eruption. Considering sex and age incidence, he gave figures from 55 cases. Of these, 15, or 28 per cent., were in males; 40, or 72 per cent., females. The difference was striking.

As to the liability of the eyes to inflammation, the right appeared most frequently affected, but he thought there was little in the observation. The decades of maximum incidence proved to be between 20 and 40 years, but in considering this it had to be borne in mind that the duration of the cases was long—it might extend to fifteen years. The features of corneal invasion were of interest. He noted the occurrence of crescentic areas of infiltration separated from the patch of scleritis by a band of clear cornea; these crescents had their concavity towards the limbus. When there were frequent attacks of scleritis in different parts of the eye, a complete ring of these small crescents might result, simulating arcus senilis. He thought they were due to obstructed lymph circulation in the cornea. He had observed similar crescents capping fascicular ulcers. The invasion of the inner parts of the eye was due to extension of the poison. Cyclitis, iritis, and opacities of the cornea were evidence of the severity of the inflammation. Coming to etiology, he found evidence of rheumatism in 24 cases, and a definite absence of it in 26. Indigestion and constipation and pyorrhoea alveolaris were exceedingly common in the affected, but were these not common in most hospital patients of this age and sex?

The preponderance of females suggested the possibility of disease peculiar to female organs; but the most careful examination of many cases brought no evidence of this. The most certain thing was that the subjects were young women of sedentary habits—milliners, dressmakers, and the like. Dealing with pathological features, sections of nodules showed no evidence of tubercle; cultures were most often sterile. In one where the *Staphylococcus aureus* was isolated a vaccine was prepared, but it proved useless. Von Pirquet's reaction was tried four times; two were positive and two negative. In the positive cases tuberculous injections were made; both recovered, though no striking changes in the scleritis appeared after the injections. Three cases were examined for syphilis by the Wassermann test, and all were negative.

As regards treatment: He had excised nodules in nine cases with varying results; on the whole he thought well of it. He suggested incising the patch and touching it with carbolic acid. Injections of mercury cyanide had proved useless. Iridectomy had been done for secondary causes; in one case the scleritis recurred four months later; in another not for several years. Hot vapour baths were very good during attacks, but did not prevent recurrence. It was too early to estimate the value of ionization; he found the reaction severe. It tended to

increase the severity of the attack, but one case appeared to be improved. He was inclined to consider it too painful. Salicylic ionization seemed much more pleasant. In his opinion the disease was toxic, that is, not due to a local organism; and he based his opinion upon the absence of organisms in the nodules. He thought it was a local thrombosis, something like a chilblain, not that it was a cold weather disease or that warmth was a great factor in relief. It was a toxic thrombosis, and possibly the manifestation of tubercle in other parts of the body; yet it was not limited to tubercle. At present they had no means of distinguishing these toxins.

II.—Stephen Mayou, F.R.C.S.,

Senior Assistant Surgeon, Central London Ophthalmic Hospital.

It would be impossible to discuss the whole of such a large subject in the time given to it at this meeting. I therefore propose to confine my remarks mainly to the bacteriological aspect of chronic cyclitis. During the last five years I have had some 38 cases under my care, upon which these observations are mainly based. During the first two years the late Mr. Hancock gave me his valuable assistance in this investigation, some of the results of which have already been published in the *Ophthalmoscope*. There are many deficiencies in the proof of some of the suggestions put forward in this paper, and if some of the statements appear dogmatic I would plead in excuse that it is with the idea of raising discussion.

It is only within the last few years that any attempt has been made to arrive at a scientific diagnosis of the cause of chronic uveitis. Indeed before this time syphilis, and occasionally tubercle, were the ascribed causes, and such cases were treated empirically on their clinical manifestations. I hope to be able to show that some pyogenic organisms may also cause a non-suppurative inflammation of the uveal tract, and can be obtained in the aqueous in those cases. A number of biochemical and tissue reactions have been comparatively recently introduced which assist in the diagnosis. The use of vaccines associated with repeated paracentesis have, I think, made an advance in the treatment of this most intractable disease.

The Nature of the Infection.

Although it is possible that many organisms may be the cause of chronic uveitis, as far as my investigations go there seem to be three chief causes—namely, infection by the tubercle bacillus, staphylococcus, and the *Spirochaeta pallida*. Out of 30 cases in which there was good evidence of the cause, 15 were tuberculous, 10 staphylococcal, and 5 syphilitic.

Apart from external wounds in the globe, infection of the uveal tract must take place via the blood stream, and it necessarily follows that there must be some focus of infection elsewhere in the body from which the ocular affection arises.

The above organisms causing the uveitis have all been demonstrated in the aqueous by different observers. There is no evidence that a toxin alone circulating in the blood can cause inflammation of the uveal tract.

In the tuberculous cases under my care the primary foci were most frequently either in the lung, glands in the neck, or mediastinum. In all the cases there was little evidence of activity in these lesions, which in nearly every instance was a small one.

The staphylococcal lesions which gave rise to ocular affections were usually chronic ones, such as pyorrhoea alveolaris, boils, and leucorrhoea. In a few cases no primary lesion could be found, and in these cases the intestinal tract may have been the source of the infection, or, as is more probable, the original lesion had disappeared, leaving only the ocular manifestations.

Chronic middle-ear disease has been recorded in some instances, but the latter more usually gives rise to a suppurative inflammation, for the reason I shall point out. Although the staphylococcal form of the disease is really of the nature of a very chronic pyaemia, as yet no attempts have been made to demonstrate the organisms in the blood; although the organism has been found in the blood of patients suffering from the associated lesions, such as boils, pyorrhoea alveolaris, which probably accounts for the toxaemic cachexia which so frequently accompanies these diseases.

All my cases of septic origin occurred in females. Whether this is merely coincidence I am not prepared to state, but I think the disease is more prevalent in women.

The probable reason why the primary lesion is of a suppurative nature and the cyclitis non-suppurative is that the organisms become decreased in virulence from their contact with the blood serum, and also to the fact that the organisms are probably free in the blood and are not contained in an embolism or blood clot, and therefore only comparatively few lodge in any individual part of the uveal tract. In contradistinction to this is the cyclitis associated with mastoid suppuration, which is always suppurative; the jugular vein being plugged by a mass of infected blood clot, a portion is carried to the eye by the blood stream, and there forms a local focus containing a large number of virulent organisms which have not first come in direct contact with the blood serum.

Endogenous infections of the more chronic type involving the uveal tract, more especially tubercle, frequently affect both eyes. This seems to be due to the selective action of bacteria to grow more readily on the same soil as that on which they were originally cultivated. Thus, in the case of the tubercle bacillus, it is well known that it will pick out all the joints of the body; so in the same way it may affect both eyes. If, therefore, an eye has become disorganized by tubercle it ought to be removed, as it is a danger to the other eye. Going a step further, if we regard sympathetic cyclitis as being due to a septic infection of the exciting eye, it may be inferred that a chronic pyaemia is set up which causes an infection of the other eye through the blood stream without producing other infective lesions, as the organism has no suitable similar soil on which to grow.

The Intraocular Manifestations.

I do not propose to go into the clinical and histological changes in cyclitis which are so well known, but rather to touch a few special points.

Keratiis punctata consists of the round cell exudation which accompanies all chronic inflammation, being deposited on the back of the cornea. It may be derived from any part of the uveal tract, even from lesion of the choroid near the posterior pole of the globe, the cells making their way forward through the vitreous and suspensory ligament into the anterior chamber. The size of the deposits varies somewhat with the variety of the infection.

In staphylococcal and syphilitic cases it usually consists of a cloud of fine dots which may cover the whole of the back of the cornea, whilst in tubercle the cells tend to collect in large masses which are known as "mutton fat K.P." This does not depend on the amount of exudate, since it may be greater in staphylococcal lesions, but I think is probably due to the agglutination of the cells owing to variety of agglutinin body present in the aqueous.

Interstitial Keratitis.—All forms of cyclitis, especially in their subacute stage, may be accompanied by an opacity in the deeper layers of the cornea. In tuberculous cyclitis the opacity consists of localized nodules of round-celled infiltration in the deeper layers of the cornea. In syphilis an interstitial keratitis may result. In staphylococcal cyclitis a condition known as keratitis profunda may arise; in the latter condition the round cell infiltration is not so large in amount, but more fluid is effused, often giving rise to wrinkling of Descemet's membrane. Descemet's membrane being impervious to organisms it is probable, at any rate in the early stages, that the corneal infiltration is due to the diffusion of the toxins in the aqueous into it; in the case of syphilis this has been proved by the injection of an emulsion from a primary chancre into the anterior chamber, which causes an interstitial opacity; in the case of tubercle, and probably also of syphilis, secondary invasion by the organism may take place from the limbus.

The changes in the iris are essentially those of atrophy following the vascular sclerosis secondary to the inflammation. There is discoloration due to the loss of pigment of the stroma; this I have been able to verify in a piece of iris removed by iridectomy in a case in which extraction of a cataract was performed. In two cases of subacute cyclitis with a quantity of fine K.P. and keratitis profunda,

in which the staphylococcus was found in the aqueous, localized patches of complete atrophy took place in the iris stroma near its root. It is possible that these may have been the seat of the infection.

The cause of the lenticular opacity, as has been pointed out by Mr. E. T. Collins, is probably due to the action of the toxin on the cells lining the lens capsule, causing their death, which allows osmosis of the aqueous to take place into the lens, so that it becomes opaque.

The manifestations in the choroid are of two types: They may be entirely limited to the anterior part of the choroid and are then due to the direct spread of the disease backwards from the ciliary body, or isolated patches may occur near the disc, which must be regarded as separate foci of the disease. The latter are of interest in that they may be of help in the diagnosis. Taken as a general rule, there is less pigmentation in the tuberculous form than in the syphilitic or septic forms.

In two cases where the staphylococcus was found in the aqueous, in one of which the Wassermann reaction was tried and found negative, there was intense pigmentation in the fundus similar to that found in syphilitic cases, except that the patches were localized and not scattered all over the fundus, as is more common in the latter disease.

Two cases of tuberculous origin, and one of staphylococcal, in which nodules were present in the choroid near the disc, and which were under observation for two, three, and five years, exhibited from time to time a change which is probably more frequent than is usually recognized. The patches, after they had become quiescent from the original attack, showed an atrophy of the choriocapillaris, allowing the large vessels in the choroid to be easily seen; their visual acuity was normal. These patients were subject to attacks of misty vision, and, when examined, the patches were seen as white areas, probably due to subretinal oedema. When this disappeared, the vision became restored. One of these patients had as many as ten attacks, but the size of the atrophic patch did not increase or the vision diminish. Presumably this oedematous change was due to some local vascular sclerosis following the inflammation rather than a recrudescence of the disease.

The Variations in the Tension of the Eye.

There is little doubt that in some cases the albuminous nature of the aqueous inhibits its filtration from the angle of the anterior chamber, and it becomes deepened, and the tension raised. But in some cases, especially in sympathetic cyclitis, the anterior chamber is shallow, and the angle of the chamber occluded. In these cases the alteration in the vitreous seems to play an important part. As a general rule, eyes submitted to histological examination are fixed in formalin, which does not coagulate the vitreous. Recently I have been fixing eyes, where it was desired to examine the vitreous, by boiling, which coagulates the vitreous. In two exciting eyes of sympathetic cyclitis, in which the tension was raised, the vitreous was found to consist of a dense albuminous coagulum. Probably as a result of the alteration in the osmosis through the hyaloid membrane and the increased absorption of water by the albuminous vitreous the lens and iris had been pushed forward, the latter blocking the angle of the anterior chamber.

Atrophy of the ciliary body has always seemed an inadequate explanation of the diminution of tension in the eye. I have only had the opportunity of examining one case of very chronic tuberculous cyclitis in which the tension of the eye was low and yet the eye not disorganized. In this case, although the retina was for the most part in its normal position, the hyaloid had shrunken into a clear membrane behind the lens; the vitreous having been absorbed, the space between the hyaloid and the retina was occupied by a clear comparatively non-albuminous fluid. It may prove on further investigation that in many instances the so-called fluid vitreous, which so frequently accompanies these cases, may not be vitreous at all, but merely fluid between the shrunken vitreous and retina. It is easy to see that the tension in such an eye can be readily disturbed, and is dependent on whether more or less fluid is absorbed by the retinal lymphatics, and the cause of retinal detachment which may take place.

The Means of Diagnosis.

The clinical appearance of the eye is of extreme importance in making the diagnosis. In tubercle the mutton-fat K.P. and the presence of nodules on the iris, perhaps associated with tuberculous lesions elsewhere, is almost diagnostic. On the other hand, where the cornea is covered with fine K.P., with, perhaps, some deep infiltration of the cornea (keratitis profunda) associated with pyorrhoea alveolaris, a staphylococcal lesion must be suspected. Iritic adhesions may be present in all forms of the disease, although they seem less common in the staphylococcal form.

Vitreous opacity is not nearly so dense in the tuberculous form, and may be absent. It is always present in the syphilitic and staphylococcal forms of the disease, especially in the latter. There are a number of cases of cyclitis which, when they present themselves for examination, it is impossible to say from the clinical appearance whether the infection be tuberculous, septic, or syphilitic in origin. In these cases reliance must be placed on various tests.

The first group of these is to determine the presence of the organism within the body. The value of these is principally by a negative reaction to exclude the disease. Thus von Pirquet's reaction for tubercle is given so frequently in apparently healthy people as to make its utility doubtful. Since its introduction this reaction was tried in most of my cases, but two of the cases which yielded this reaction proved subsequently to be staphylococcal in origin.

The Wassermann reaction has only comparatively recently been introduced, and as yet personally I have not had sufficient experience to justify any statement as to its value in the diagnosis of syphilis in cyclitis. I have used it in the later cases to exclude syphilis. The early cases were first submitted to a course of iodide with the idea of excluding this disease. The local reaction of the eye following the injection of the toxin into the blood is a most valuable means of diagnosis. The reaction usually shows itself about forty-eight hours after inoculation by increased ciliary injection, K.P., and vitreous opacity. These generally render themselves obvious to the patient by a slight impairment of vision. If active choroidal mischief is present in the fundus the patches show increased oedema. These changes are usually produced after the first injection if the dose given be sufficiently large, but occasionally they do not make their appearance till the second injection. The dose required to produce the reaction is nearly a full one. Such is administered in treatment—for example, tuberculin T.R., $\frac{1}{1000}$ mg.; staphylococcus, 1,000 millions.

The bacteriological examination of the aqueous is the only means by which absolute proof of the nature of the organisms can be obtained. Probably owing to the scarcity of the organisms, personally I have never been able to find organisms in smear preparations of the aqueous, even after centrifugalization, although cases have been reported in which the tubercle bacillus has been found. The inoculation of the aqueous into animals from a case of tuberculous cyclitis has been followed by positive results. The aqueous in tuberculous cases will also yield the Bordet reaction. In septic cases the method which I have adopted is to inoculate the aqueous on to agar or blood serum. It is obvious that the question of contamination from the conjunctiva must be excluded.

The method adopted in the early cases was, after carefully cleansing the conjunctival sac by frequently irrigating it for three days, the anterior chamber was tapped with a keratome, and as the fluid escaped over its surface it was drawn into a sterile capillary pipette and the media inoculated therefrom. Although the risk of contamination is extremely small, to exclude all possibility of outside contamination in 3 cases a hollow needle with a spear-like point, as has been suggested by Mr. Bishop Harman, was used, the point of entry being first touched with the cautery. The fluid was withdrawn into a sterile syringe and inoculated on to the media. In all these cases the *Staphylococcus albus* grew on the media, and in one instance as many as twenty colonies were present, although, as a rule, there are not more than three or four. I do not adopt this method as a routine, as it is more dangerous than simple paracentesis; indeed, if the needle is not very sharp, and the blade sufficiently broad to make

an incision large enough to admit the shaft, great difficulty may be met with in its introduction, although personally I have had no accidents.

Treatment by Repeated Paracentesis and the Administration of Vaccines.

The routine which I generally adopt on seeing a patient with cyclitis of doubtful origin is to do a Wassermann reaction to exclude syphilis. Von Pirquet's reaction is then performed and if negative the anterior chamber is tapped and cultivation made. If the staphylococcus is found a vaccine is made and administered. If von Pirquet's reaction is positive a dose of tuberculin is given and the local reaction watched for. Mr. Hancock and myself in 1908 published a series of tuberculous cases, both of scleritis and cyclitis, treated by tuberculin T.R. and the beneficial results obtained by it. In the early cases the opsonic index was carefully watched, but I think it is unnecessary since the local reaction in the eye gives a more accurate guide to the effect of its administration and the dose can so be regulated. In patients who had no extensive tuberculous lesions elsewhere $\frac{1}{1000}$ mg. was first administered, and this gradually increased up to $\frac{1}{250}$. In patients who had other extensive tuberculous lesions $\frac{1}{5000}$ was administered, and this often cannot be increased as the local reaction produced is severe, probably owing to auto-inoculation. The vaccine is administered by hypodermic injection into the arm.

Of 15 cases so treated, in 12 the eye or eyes became quiescent, the K P. disappeared. As some of the cases were more advanced than others, the amount of vision regained depended on the stage of the disease in which the patient sought treatment. In one of the cases in which both eyes were affected the disorganized eye was removed.

Three of the cases were not benefited by the treatment, although one showed an improvement for a short time. One of these was in a patient with buphthalmos, and another in a very debilitated woman. One child subsequently developed other multiple tuberculous lesions, but not till eighteen months after the eye had become quiescent and tuberculin treatment had been stopped.

The *rationale* of treating localized septic inflammatory conditions elsewhere in the body is to evacuate their contents, thereby getting rid of the organisms, toxins, etc., and also to allow the site of the lesion to be flooded with blood serum containing protective bodies. In treating cyclitis of septic origin I have tried to adopt these principally by performing repeated paracentesis of the anterior chamber, and combining it with the administration of vaccine, so as to raise the amount of protective bodies in the blood serum. After evacuation of the anterior chamber these bodies are found in the aqueous. An initial paracentesis is performed to obtain the organism with which to make the vaccine which is administered. Ten days later, when the opsonic index is at its height, another paracentesis is performed, which can be repeated after subsequent inoculation if desired. In 1908 I published a series of cases so treated. The good results have been fully maintained. Out of 10 cases, in 6 the K P. cleared entirely, although in most a few vitreous opacities remained. In 2 cases one eye cleared, whilst in the other K P. is still present, although the eye shows a marked improvement. In 2 a marked improvement as regards vision occurred, although the K.P. was still present when last seen. The cases seen in the early or subacute stages yield more readily to treatment, although in one case, in which the disease had existed for over two years, and the vision was reduced to $\frac{6}{60}$, it improved to $\frac{6}{6}$ in eight weeks. Old-standing cases, with marked hetrachromic, deep anterior chambers, and commencing lenticular opacities, improved very slightly under treatment. The K P. occasionally disappears soon after the paracentesis, but may reform. Wrinkling of Descemet's membrane and slight infiltration of the deeper layers of the cornea may occur after the operation, but subsequently disappear.

In most of the cases a vaccine was prepared from the eye, but in some instances a mixed staphylococcal vaccine was used. Although the latter was efficient the former is more satisfactory; indeed, in one case in which the mixed vaccine was tried and was unsuccessful improvement took place when the patient's own organisms were administered. To summarize the result of the treatment, I should say the method is a distinct advance in treatment as yet far from perfect. In conclusion I have to thank my colleagues of the Central London Ophthalmic Hospital for their help and for allowing me to use some of the cases under their care.

DISCUSSION.

Mr. ARTHUR W. ORMOND (Guy's Hospital) said the main difficulty in these cases lay in the etiology, and, therefore, in the treatment. Syphilis, acquired and chronic, gonorrhoea, rheumatism, tubercle, malaria, leprosy, dental caries, Bright's disease, were all well recognized causes, and many other septicaemic conditions might also be associated with irido-cyclitis, but there was a large number of cases in which the etiology was far from obvious. Maitland Ramsay, among others, had suggested that some of these cases were due to faulty metabolism, and no doubt a large number of cases of uveitis which had been attributed to gout and rheumatism were really due to impurities absorbed from the alimentary canal; probably the prevalence of these diseases at certain periods of the year might be the result of certain foodstuffs then in season. It was generally accepted, however, that irido-cyclitis was often the effect of the general infection of the blood by micro-organisms or toxins; this being the case it seemed rational to examine the blood for evidence of change. The speaker had recently had three cases of sympathetic ophthalmia under his care in Guy's Hospital, and Dr. Pryce Jones had examined the blood of these patients on several occasions. The very marked change which he found consisted in a tremendous increase in the percentage of mononuclear leucocytes. Ehrlich had given the percentage in normal blood as from 2 to 4 per cent., whereas in these cases it ranged from 16 to 20 per cent., which suggested a protozoal and not a bacillary infection. This was shown in the following table. In

Blood Examination of Three Cases of Sympathetic Ophthalmia.

	Lympho-cytes.	Mono-nuclear.	Poly-nuclear.	Eosin.	Mast.
	Per Cent	Per Cent.	Per Cent	Per Cent	Per Cent.
Normal (Ehrlich) ...	22-25	2-4	70-72	2-4	0 5
Case 1.					
February 7th, 1910 ...	33.6	20.2	40 8	4.2	1.2
February 9th, 1910 ...	21.6	20.5	52.6	4 6	0.5
February 28th, 1910	33.3	17 8	45.0	4.4	0.8
Case 2.					
March 16th, 1910 ...	21.6	17.3	57.3	3 3	0.3
March 17th, 1910 ...	43.6	17.3	35 0	3 3	0.6
April 7th, 1910 ...	35.0	13 6	46.0	4 3	1 0
Case 3.					
June 23rd, 1910 ...	21.0	16.0	58.3	4.0	0.6
July 12th, 1910 ...	32.0	7.0	58.3	2.0	0.0
July 18th, 1910 ...	43.3	6.3	47.0	2.3	1 0

In Case 3 between the first and the second examinations an iridectomy had been performed, and large doses of salicylate of soda given, and the patient's condition had very much improved.

sympathetic ophthalmia it was constantly noticed that the condition in the sympathizing eye at once improved when the exciting eye was removed. This improvement might, however, be only temporary; it at least suggested that the exciting eye had a direct and close influence upon the sympathizing eye. This point had lately been called in question by Mellor of Vienna, who, arguing from the existence of sympathetic ophthalmia in cases of intraocular sarcoma—that is, in cases where there was no possible traumatism in question—suggested that the infection might sometimes be endogenous and not exogenous, and that the organism producing sympathetic ophthalmia did not always reach the exciting eye through the perforating wound, but might sometimes do so through an abrasion of the skin or mucous membrane. No very satisfactory explanation had ever been offered to account for the sudden lighting up of sympathetic trouble in a case where there had been no sign of any active mischief for five, ten, or even more years, except that brought forward by Mr. Hutchinson—

namely, that the organisms were encapsuled, and so remained in the tissues in a harmless condition until something happened to disturb this encapsuled condition and they became active once again. To the speaker himself the theory of a fresh infection through skin or mucous membrane was more attractive than the idea of their encapsulation. The frequency with which the aqueous removed from a case of sympathetic ophthalmia was found sterile suggested that toxins were sometimes the actual irritant, and that the organism might be acting at some distance from the eye. With regard to treatment, he would strongly urge the claims of paracentesis and the irrigation of the anterior chamber with normal saline solution; he found this answer excellently in cases with increased tension, and where the iris and filtration angle were clogged with inflammatory débris. The condition was analogous to the formation of an abscess elsewhere in the body, and the same principle of treatment should be adopted—incise the abscess and evacuate the pus; the same idea held good in many cases of irido-cyclitis, and an instance of this he had recently had. A woman, about 50 years of age, had a dense deposit in the anterior chamber from irido-cyclitis, and also increased tension. She absolutely refused to allow any surgical interference whatever, and this deposit was watched gradually making its way through the cornea, which, after three months' time, had a large ulcer on its surface, through which the deposit was discharged; when this had cleared, the eye got quite well again, except, of course, some opacity in the cornea.

Dr. W. H. Brailey (Brighton) quoted a case of uniocular irido-cyclitis, which was probably due to the sting of an insect on the side of the nose. He commented on the difficulty in obtaining certain evidence of rheumatism, and the fallacies of statistics as indicative of the prevalence of any chronic disorder in males and females, owing to the fact that men did not trouble so much about these conditions.

Dr. R. A. Reeve (Toronto) commented upon a case of homolateral irido-cyclitis consequent on follicular tonsillitis. First one side of the throat was affected and the corresponding eye; later, the other side of the throat and the other eye. He had frequently noted correspondence of tonsillar affections with the eye inflammation.

Mr. Hill Griffith (Manchester) said that when using the cautery in cases of scleritis he had found pus in some of the nodules; he thought solid nodules were rare. Commenting on the peculiar corneal scars left in scleritis, he asked if they were analogous to the white corneal lines so frequently seen in dermoid of the limbus. In some cases the interval between the attack of the two eyes extended to many years. He noted that Mr. Mayou accepted the explanation of the occurrence of keratitis with solitary patches of choroiditis he had put forward some twenty-three years ago.

Dr. G. A. Berry (Edinburgh) said the proof of toxic origins of inflammation was much more difficult than the detection of the presence of organisms, and this should have some influence in their judgement of difficult cases. He thought that tuberculous irido-cyclitis had many features in common with sympathetic ophthalmia; the latter might almost be analogous to a malignant form of the former. As regards staphylococcal infections, he was strongly of the opinion that the soil in which these were found as apparently causal agents was prepared by tuberculous infections elsewhere. He agreed in the preponderance of cases in women, but he would put the percentage as high as 90 per cent.

Mr. N. Bishop Harman (London) said, with the aid of Dr. Bernstein, he had investigated a large number of eye cases by the Wassermann reaction. He was much impressed by its reliability. Citing the case of a large family, of which only two members remained, both these women suffered from forms of chronic uveitis. One had scleritis during twenty years, the other posterior inflammation. With the introduction of the test they were each examined twice, and gave a positive reaction. Another woman with granuloma of each iris and the "mutton-fat" variety of K.P. had been thought to be tuberculous, but the Wassermann reaction was positive, and mercurial treatment proved much more beneficial than tonics. It must be borne in mind that the test only signified a general taint; it did not prove that a particular lesion was syphilitic; for this the test must be standardized. His experience of paracentesis was most happy, and in this connexion it was of great interest to read the ancient experiences of Wardrop.

Dr. Cecil Shaw (Belfast) expressed his approval with Mr. Spicer's views that many of the cases of sclero-keratitis were toxic. He had been struck with the number of patients who seemed otherwise healthy, but who led sedentary lives without sufficient open-air exercise—for example, domestic servants. As regards foci of infection in uveal inflammation, he thought the nose should be carefully examined, as it was probably a frequent source of septic infection. Mr. Mayou had referred to boils as primary foci, but he thought both boils and the eye infections were secondary.

Mr. Holmes Spicer and Mr. Stephen Mayou replied.

DRAINAGE AFTER CATARACT OPERATIONS.

By G. H. Fink, M.R.C.S., L.S.A.,
Major, Indian Medical Service (ret.).

[Abridged.]

After speaking of the advantage of efficient drainage in surgery the author, speaking of cataract operations, said: We have to consider whether the drainage in the after-treatment and while at rest in bed is efficient; and whether we cannot produce more efficient drainage to advantage. These are two points of much importance which I will take up and endeavour to show the advantages which can be gained by the assistance of the patient as well as of the operator.

As a rule, the recumbent posture in bed with the head resting on a pillow is one of great importance for the operator to show the patient for the first twenty-four hours after the operation. Some prefer a high pillow, others a low one. What is required is a semi-recumbent position for the first few hours, for the simple reason that the processes of healing are occurring, and there is a certain amount of plastic material being thrown out from the blood, while there is a certain amount of lacrymation occurring to effect drainage.

Healing by first intention is what is aimed at with a perfect coaptation of the edges of the wound, and if posture is necessary to pay attention to in a broken limb, it is also necessary with an eye which is injured or operated on. I maintain that the semi-recumbent position for the first few hours aids Nature in the healing processes, while the complete dorsal decubitus has two grave disadvantages, (*a*) the nervous class of patient who lies flat on his or her back after the operation often has an over-rapid heart action when the blood is driven towards the great arteries and flows with a rapid flow which disturbs the repose of the circulation in the eye so necessary for healing; and if there happen to have been some accident during the operation, as sometimes does occur, then there is graver danger still of the non-union of the wound by first intention. (*b*) With the head thrown back on a low pillow, the stream of discharge flows towards the conjunctival sac under the upper lid; when this is full, the stream flows on either side towards the inner or outer canthus. It is better to prevent this overflow of the upper conjunctival sac, and to instruct your patient to lie on the side of the outer canthus where the flow gravitates and gets absorbed by the dressings and cannot be retained, and this position is what is necessary to adopt after the first few hours subsequent to the semi-recumbent position.

It is a curious fact that in India the patients who were anaemic, and whose eyes were operated on, not only had the wounds rapidly healed by first intention, but there was no redness of the conjunctiva or any indication of any complication of any kind after. The circulatory organs and the blood, therefore, have some influence in this kindly state of things, and it somewhat explains what I have stated above under (*a*) and (*b*). Within twenty-four

hours frequently the opposite sides of the linear incision have united.

The dressings usually employed in the toilet after cataract operations are the pad and bandage. I have used these in my cases which amount to over 1,000 by the eclectic method; and in India I have found the moistened pad better in the hot season, for it is cool, and it has another advantage, that it prevents the patient from trying to shift it, which, in a hot climate, is unconsciously done in some cases. In the cold season the dry pad and bandage is better in my opinion.

But in the majority of my cases I found the antiseptic plaster which I have employed, and which is described in my pamphlet on "The Methods of Operating for Cataract, etc.," most useful in patients in India. I feel that it has special claims to recommend it in the hot months particularly, and for the reasons I am about to adduce, for the majority of patients even in the cold season or winter.

1. It is most easily applied as an antiseptic and cool non-irritating dressing over the closed lids.

2. It prevents patients from trying to remove the dressing, which happens in some cases where the pad and bandage are used, and are either very uncomfortable and hot, or the patient and friends sometimes are anxious to know whether the eye is able to see. Such interference is rendered difficult by the plaster adhering to the skin firmly and is more troublesome to remove.

3. It does not prevent the application of the pad and bandage over the plaster, which, in some cases where escape of vitreous has occurred, may be necessary as an additional precaution.

4. It has two slits or openings at the inner and outer canthus to admit of drainage.

5. It acts like a splint to the eye.

With regard to (4)—that is, the slits for drainage—although this is fairly efficient, I think it is possible to improve in this part of the finishing toilet in our cases, and with greater advantage too, by the introduction of a small india-rubber drainage tube of fine calibre (either round or triangular) with about three to four small openings in the centre pointing upwards towards the linear incision. Such an india-rubber tubing can easily be borne between the upper and lower lids and along the tarsal margins. It in no way creates discomfort in any sense, and if placed as given in the diagram below, it not only permits

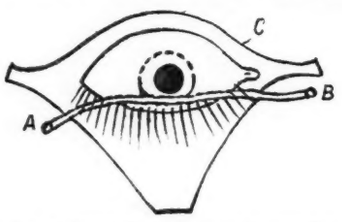


Fig. 1.—A and B, Exits of drainage tube inserted between upper and lower lids and slightly within the upper. C, Plaster over the closed eyelid and drainage tube.

of drainage but it also allows the introduction of a suitable antiseptic lotion gently syringed at the outer end of the india-rubber tube, which irrigates the whole of the surface of the wound and washes it out thoroughly, while the discharges are drained out at the inner canthus. This can be done most efficiently while the patient is lying down, and without removing any dressings which take up much of one's time.

It further admits of any particular medication necessary should there be pain within the twenty-four hours, without the necessity of removing the dressings.

We all know the value of an opiate in major operations at bedtime. It is the routine treatment after surgical operations of any consequence unless contraindicated, and is beneficial. Opium or morphine, in cases after cataract operations, is not administered, unless very exceptionally for particular reasons. The effect of this drug on the pupil is to cause contraction Some operators use eserine drops after their operations for cataract, and it is doubtless necessary in certain cases where the pupil has been very much dilated, or there is danger of it being caught between the lips of the incision, creating a disfigurement later in the appearance, and, in some cases, also prevents healing by first intention. But, on the whole, a fairly dilated, not an over-dilated, pupil, after cataract extraction or expression, admits of better drainage within the first few hours should there have been any complication, or accident, or bruising of the iris while operating. The irrigation of the anterior chamber gently after cataract extraction has been found to be useful in its results, and I feel, therefore, that the introduction of efficient drainage in the manner mentioned and for the objects stated would promote cleanliness and thereby healing, besides proper medicament, if necessary, without disturbing your patient, by the india-rubber tube of fine calibre within the margins of the closed lids.

Very often the lids get firmly glued together with discharges, especially in a certain percentage of straightforward cases without any complication whatever, and also in a certain percentage of unfavourable cases. There is danger attending such a condition because the secretions and discharges get pent up within the delicate folds of the conjunctiva and prevent healing, besides causing a discomfort and pressure on the eyeball. By the introduction of the fine drainage tube with openings in the manner mentioned such a contingency is greatly averted, and adds to the patient's comfort as well as the operator's in case of any medication of the eye without disturbing the patient by removing the dressings.

It saves much time and labour, besides enabling the operator, if he wish, to examine the character of the discharge from the eye microscopically by syringing out the discharges very gently. This could be effected by inserting the nozzle of a glass syringe, and of the tube shown in the figure.

No special apparatus is necessary beyond the drainage tube of fine calibre and a glass syringe with a nozzle to fit into the drainage tube at A, and, by gentle syringing, irrigate the eye, driving the necessary antiseptic or medicament through the tube which passes through the openings in the centre of the tube, washes out the folds of conjunctiva and the globe of the eyeball, and returns through B. After the tube has been placed in position and held fast by the antiseptic plaster which is laid over the lids in the final stages of the dressing, the ends are passed through two slits in the plaster at the inner and outer canthus respectively, and the ends gently filled with a plug of antiseptic iodoform cotton-wool as a filter. This plug can be inserted with a fine silver probe, and removed when necessary for the purpose of syringing.

SCHOOL CLINICS.

By N. BISHOP HARMAN, M.A., M.B., B.C.Camb., F.R.C.S.Eng.

BEFORE the conclusion of the meetings of the Section, Mr. Bishop Harman brought forward a motion relating to the establishment of school clinics, which ran as follows:

> In view of the importance of obtaining continuity of treatment of defects of vision in school children throughout the period of education, and of proper co-ordination of medical inspection and treatment, it is the opinion of the Ophthalmological Section of the Association that the organization of school clinics is desirable.

In support of the motion, Mr. Bishop Harman said: The question of school clinics is at this moment a matter of agitation in municipal circles over the whole country, more acutely in some places than in others. In this, so far as it relates to party politics, we have no concern; but we are concerned with the treatment of the vision of school children in so far as it touches medical practice, the welfare of the child, and the by no means unimportant matter of scientific data concerning vision and the effect of school life thereon.

Since, as members of the British Medical Association, we are interested in economics, we shall not be content with any mode of dealing with this question that is not just between the man who does the work and the State that requires the work to be done. We should clearly set before us this fact, that the treatment of school children as members of a school is not charity but a public service, and as such is to be met by a just remuneration for service rendered, as is the service of the builder who repairs the fabric of the school building. This point need not be elaborated, but it should be borne in mind in any representation that we may make to the Council of our

Association, not that we need fear they are unmindful of it, but to strengthen their hands.

The second point is the economy of time; it is one that concerns us as hospital doctors, perhaps as school doctors, and all of us as citizens. At present much money is expended on medical inspection. This is good work; it is done so that public money spent on education shall not be wasted in inefficient children. So far as my experience goes—and it is a large one for London—the work is done well. But this work is waste of time, energy, and money unless it be promptly followed by efficient treatment.

In the past there have been attempts to turn the whole matter over to the hospitals. But the attempt has failed both because the hospitals were unable to meet this huge addition to their already heavy labours, and also because it was recognized that charities were not the places for fulfilling State requirements.

At the present time there are tentative arrangements being made to meet this necessity for treatment both by private and by public arrangement, and from both of these we are gaining experience. Public arrangements have been made in London and are now on trial. Certain hospitals have agreed to treat certain numbers of children for which capitation grants are made, and also certain grants for the payment of the hospital officers who do the work. Of these arrangements themselves nothing need be said, but the working of them may be examined. One immediate result of the scheme has been that many hospitals will no longer treat refraction cases in school children; they are refused with the intimation that special arrangements are made for their treatment. Parents, most of them poor, often very poor, lose time and wages in attempts to fulfil the demand that their children's defects should be remedied. The following example is no isolated instance of the hardship worked: A father receives a notification for the treatment of his child. He takes the child to a hospital; he is refused there, referred to the special school department; there he is not accepted because he has no voucher, so his visit is wasted; procuring a voucher from the school teacher, he makes a new visit to the appointed place, and then one, two, or perhaps three visits are required for treatment; then there is the business of getting glasses. At least four half days'—and in some trades that means whole days'—work are lost to father or mother, and the cost to the family in wages is out of all proportion to a consultant's fee of two guineas to the well-to-do.

There are a few school clinics already established in different parts of the country. Some are by private, others by public, arrangement. The convenience of their management is in great contrast to the cumbrousness of the hospital *plus* public authority scheme. In these districts children requiring treatment are visited by a specially appointed and paid doctor at their school or at some convenient centre. The work is done on the spot and the supply of glasses is arranged for.

One criticism of the utilization of hospitals—at least, so far as large towns are concerned—is that these institutions are situated in the centres of cities, often several together, and not in centres of population where the children live and go to school. The many difficulties of the hospital scheme and the simplicity of the school scheme are in such contrast that elaboration is not needed.

Lastly, by our present arrangements much of the value of our work to us as investigators is lost, and the loss extends to the schools. Much of the value of medical inspection and medical treatment of school children will accrue in a communal sense as distinct from the benefit to the individual child. We aim at preventive measures; we want to find out causes of defect or influences that increase existing defects. Were the treatment of these children done in some orderly fashion it would be possible to correlate the conditions of the vision of children with different modes of education, different modes of life, varying conditions of well-being, variations in parentage, each and all of which have been asserted to have important influences in the production or accentuation of visual defects which ultimately reduce the efficiency of our future citizens. At present we know nothing of these things except by the laborious efforts of private individuals in isolated regions, and without some system of co-ordination and arrangement, the allocation of places for treatment to given areas, and the continuity of treatment such as clinics would afford, this necessary knowledge is unattainable; so that consequently a great part of our work, possibly that which would prove most fruitful of benefit to the community, is dissipated. The opinion embodied in the resolution is the outcome of personal experiences and those of others familiar with similar work, and it remains to be seen whether it also commends itself to your collective experience.

The resolution was seconded by Mr. Walter Edmunds, and, after discussion, was passed with one dissentient, the whole number of members present being about 35.

SECTION OF PHYSIOLOGY.

Professor W. H. Thompson, M.D., Sc.D., President.

PRESIDENT'S INTRODUCTORY REMARKS.

PHYSIOLOGY AND SCHOOL EDUCATION.

After some preliminary observations, Professor Thompson said: Fifteen years have elapsed since the British Medical Association met last in London. The Section of Physiology was then presided over by Dr. David Ferrier, who took as the subject of his introductory remarks, "The Relations of Physiology and Medicine." In that short address, which might well be repeated to-day, Ferrier illustrated one of his points by reference to the investigations of Pavlov in St. Petersburg, which were then beginning to attract the attention of physiologists in this and other countries. Knowledge of that work is no longer confined to the ranks of physiologists. It is widely known throughout almost every branch of the medical profession. But I do not intend to dwell upon it at any length; I merely wish to refer to Pavlov's conception of the relation between Medicine and Biology. It is expressed as follows:

> In spite of the extraordinary complexity of biological processes when compared with other natural phenomena, in spite of the difficulty of bringing them under control by determining their proper causal relationships, medicine from the beginning has been destined by the inevitable ordinances of life to achieve mastery over biological things, and this even before such things became matters for scientific investigation.
>
> Moreover, medicine has, in large measure, fulfilled what was expected of her. Her task seemed endlessly large and hopelessly complex, but yet it is in no small part accomplished. From amongst the countless number of possible solutions of problems set to her she has fortunately arrived at many correct ones. This unexpected result has only been made possible by the co-operation of two conditions. These are, that man from the earliest times has constantly and passionately striven to maintain life and health; and, secondly, that in this search for health numerous individuals—indeed, I might say, all mankind—have taken part. (*The Work of the Digestive Glands*, Second English Edition, p. 243.)

To Pavlov's statement general assent will, I think, be accorded. Medicine has been driven into the study of biology, and, having learnt the solution of her difficulties, has emerged an altered science. In the early study of this subject she has discovered the rudiments of a new doctrine—that of the science of right living, of hygiene, of preventive medicine. Much the same thought was expressed a good deal earlier by Huxley in the following words:

> Natural knowledge in desiring to ascertain the laws of comfort has been driven to discover those of conduct and to lay the foundation stone of a new morality. (*Collected Essays*, vol. i, p. 31.)

It needs but little consideration to convince us that in these reflections of two eminent men we have a true conception of the evolution of scientific medicine as we know it to-day. Moreover, it has always been a fortunate circumstance for the spread of the new doctrine that the self-interest of man, "the passionate desire to maintain life and health," has secured their extensive adoption. The world at large has not had to wait for the special propagation of the new faith by those upon whose discoveries it has been founded. The laws of biological conduct—of health—are taken up in a general way and applied by mankind everywhere, step by step, as they become known.

Indeed, one of the features of modern progress in these matters is the great advance which has been made within the memory of all of us. Nor is the acquisition of this knowledge left altogether to chance. For more than half

a century means have been placed within the reach of a considerable proportion of the inhabitants, not alone of these islands, but of other civilized countries, to learn something of the new morality, as Huxley has called it. And even now a widespread awakening to the importance of knowing how best to conform to the laws of biology has set in. Nevertheless, I venture to question if the measures taken in this country to spread the truth are sufficient. In Ferrier's address, to which I have referred, the following sentence occurs:

> The inability to appreciate the value of a new truth is a measure of the depth of the individual's ignorance. (BRITISH MEDICAL JOURNAL, vol. ii, 1895.)

If we apply this touchstone to the majority of our fellow men outside the ranks of the medical profession, I think we shall be forced to admit that, notwithstanding all that has been done, there is still a widespread inability to appreciate the value of the laws of biology in their application to the well-being of man. And, if so, are we not called upon to insist that greater endeavours shall be made to remove it?

If we concede this latter point, the next question must be: How is our purpose to be effected? A little consideration will answer the question for us. Hitherto our hygienic efforts have been mainly directed to secure the welfare of communities, and have been due chiefly to the instigation of experts. The individual for whose benefit they have been instituted seldom understands their importance at first, more rarely affords intelligent co-operation, and too often resents their introduction. He has not been educated to appreciate the value of the new truth. In some respects this is not surprising. The science of physiology is comparatively young, and the field of general education has been occupied by older subjects. Nevertheless, we are much behind many other countries, and there appears to be only one way to make good these deficiencies — namely, through the medium of school education.

It is in preparing for this that I venture to think physiologists can be of most use. Teachers must, in the first place, be trained on sound lines, and by helping to do this we can ensure that the next generation, at all events, shall have a more competent knowledge of how to live healthily.

I do not forget that something has been done already, not alone in England and Scotland, but also in Ireland. Indeed, in the matter of preparing teachers for secondary schools Ireland is probably foremost of the three countries. But the work must not be allowed to stop at the training of teachers, nor should it be confined to the secondary schools. We cannot be satisfied till some knowledge of biology in its application to the maintenance of health becomes part of the education of every boy and girl in our elementary schools.

This is not the place, nor is the time at my disposal sufficient, to go into the particulars of any scheme for its accomplishment. But I think we should at least insist upon as compulsory what is now the minimum of knowledge laid down in the education code for those who voluntarily take the subject of physiology. It is expressed as follows:

> No candidate will be allowed to pass who . . . makes gross errors concerning the general build of the body and the relations of its several parts, the structure and working of the heart and the main facts of the circulation, the mechanical and chemical changes of respiration, the chemical nature of food, its changes in the alimentary canal, and its use in the body.

Moreover, we cannot be content with book work alone. It will not suffice to awaken the interest of the children. To be efficient and achieve its object the teaching must be accompanied by what we call practical instruction. No doubt a certain amount of prejudice on the part of teachers and officials will have to be encountered and overcome. Many of them hold that the less physiological knowledge people possess about themselves the better. But it will, I feel sure, rapidly disappear and give place to enthusiasm in the light of fuller knowledge.

One such incident which occurred to my own knowledge in Ireland may illustrate this. In the efforts of the Department of Agriculture and Technical Instruction to provide a training for secondary school teachers in this and other scientific subjects it was fortunate in securing the services of two enthusiasts in these matters. I refer to Mr. R. Blair, now Executive Officer in the London County Council, and Mr. George Fletcher, his successor in the Dublin office. After having overcome the hesitation to adopt physiology and hygiene of some of the teaching orders who conduct a large number of the secondary schools for girls in Ireland, it became necessary to provide teaching equipments for which subsidies were granted. One item in each of these outfits was to be a human skeleton. This, however was not to be a natural one, but a model in papier-maché. Considerable delay arose in providing the model, and the instrument makers lent a natural skeleton in the meantime. This latter would not, however, be admitted to the school, and had instead to be taken by the teacher to his own rooms. After some time, when the pupils got interested in the subject, the teacher ventured one day to bring the upper limb of the skeleton, rolled in paper, to his class. The parcel was not opened till the following day, when a suitable opportunity occurred. The impression produced was favourable, and bit by bit the remainder of the skeleton was allowed in. Later in the year the papier-maché one arrived, but it was so obviously inferior to the natural one that it was not retained. Indeed, some expense was incurred to induce the makers to take it back, and, so far as Ireland is concerned, the model still remains on their hands, though the incident took place several years ago.

Nor should we forget that the present is a very favourable time for making an advance. Never was more widespread interest taken by the public in hygienic matters. We are very conscious of this in Ireland, where we have to thank the remarkable health campaign carried out with signal success by the Women's National Health Association —an organization founded not quite three years ago by Her Excellency Lady Aberdeen. A similar movement, as you are all aware, has spread to England and Scotland under the title of "National Association for the Prevention of Consumption." These movements, however, are not intended to take the place of school teaching, nor can they do so. To produce lasting results they must be accompanied and followed up by systematic educational measures.

Let me now turn to the programme of our proceedings for a few moments. I am glad to think that, although chosen entirely for other reasons, both the subjects selected for discussion by the Section are very appropriate from the point of view of the foregoing remarks. Educational measures such as I have mentioned derive considerable assistance from reliable pronouncements made on questions of general interest by recognized authorities at meetings of this kind. Nothing could be more important from this point of view than accurate information concerning the food requirements of man and how they are to be best met; nor than a knowledge of the factors which make for an efficient circulation.

I must not anticipate either of these discussions, but in connexion with that on food I should like to ask what proportion of even well educated people realize that the main function of food is to supply energy to the human machine in the sense that coal supplies energy to the steam engine? And again, how very much fewer realize the relative energy values of different articles of diet, or that the energy value of food can be as accurately measured as a yard of cloth? Nor do I think that our discussions need be trammelled by any fear that the world's wheat supply is not going to keep pace with the increase of its population. It is more likely that Buckle was right in concluding that the birth-rate is largely determined by the food supply. Moreover, it is said that, to find an outlet for the produce of its wheat fields, Canada has taken measures to educate some of the rice-eating nations of the East to the superior value of wheat as an article of diet. If this be true, Canada, at all events, does not fear a decline of wheat production.

There is one other point upon which I would like to touch, namely, our standards of food value. We have one absolute standard, namely, the *calorie*, which is used to check the tot of the balance sheet on the debit and the credit sides. But this gives no information concerning the distribution of the different items which make up the totals. We want, I am convinced, another standard, indeed, I think three other standards, as measures of what might be called nutritive effects. These should be in the first place a protein standard against which to compare the

corresponding effects of equal weights of other proteins, and similarly a carbohydrate and a fat standard.

The sooner these are decided upon the better, and possibly this Section might see its way in the near future to take the matter up with a view to arriving at some agreement upon it.

DISCUSSION ON THE FACTORS THAT MAKE FOR AN EFFICIENT CIRCULATION.

OPENING PAPER.

By E. A. Schäfer, F.R.S.,
rofessor of Physiology, University of Edinburgh.

In considering the factors which make for an efficient circulation our attention is naturally directed in the first instance to the heart and blood vessels, since the efficiency of the circulation depends immediately upon the condition of these organs. But as their condition depends in large measure upon the condition of the nervous system and this upon the state of nutrition of the body and upon other circumstances which are at present imperfectly understood, the subject which is put down for discussion is wider than might at first sight appear. I propose, however, in the meantime to restrict my remarks to the circumstances which more immediately affect the circulatory mechanism under normal conditions.

The effective action of the heart varies (1) with its rate of rhythm, (2) with the force of the individual beats, (3) with its condition of tone. All these conditions are influenced through the cardiac nerves, by the nervous system. By way of the vagi, influences pass which moderate the rate, diminish the force, and lower the tone; by way of the sympathetic, others which make the rate of contraction more frequent and the individual beats more powerful, and which increase the tone of its musculature. The proper balance of these opposite effects, in accordance with the variations in the work which the heart at any given time is called upon to perform, must be looked upon as one of the most important factors in the maintenance of the efficiency of the circulation. The influences which reach the heart by way of the central nervous system do not originate in the cardio-inhibitory and cardio-accelerator centres. These are themselves excited to action by impulses which come to them from the periphery, or from higher nerve centres, the ultimate source of excitation being in the latter case also, probably, peripheral. These impulses come from all parts: from the skin and other sense organs, from the viscera, from muscles and tendons and serous membranes, from the blood vessels, and even from the heart itself; they may or may not be accompanied by conscious sensation. According to the manner in which they affect the heart-regulating centres, the cardiac contractions are caused to vary so as to promote under the given conditions the efficiency of the circulation to the best advantage.

The action of the heart is also affected by the condition of the respiratory system, along with which may be included the tone of the abdominal muscular wall. The importance of this as a factor in the maintenance of the circulation was shown by the experiments of Dr. Leonard Hill and his co-workers. The respiratory apparatus acts by facilitating the flow of blood to the thorax, and thus to the heart. Within limits the greater the rate of blood flow to the heart the more efficient will be the circulation, for the heart is a machine which reacts to the amount of work thrown upon it, in the same way that a gas engine, by more frequent explosions of its gases, produces a greater amount of energy in proportion to the resistance which it has to overcome. But in the case of the heart the response to increase of resistance does not take the form of increased rate of rhythm; the rate of rhythm tends, indeed, rather to diminish with increase of arterial pressure. This is a regulating effect produced through the vagi. On the other hand, increase of pressure up to a certain point within the coronary arteries is a direct excitant to cardiac activity. An isolated mammalian heart which has ceased to beat spontaneously may be started again by introducing fluid under pressure into the coronary system. Such fluid need not be of a nutrient character for this result to be manifested, since it will show itself even if liquid paraffin be injected into the coronary vessels; although, for the continuance of the contractions, a saline oxygenated fluid is essential.

The condition of the blood vessels, and especially of the arteries, is scarcely, if at all, second in importance to that of the heart. Disregarding the slow rhythmic contractions which the vessels occasionally exhibit, and which may to a slight extent aid in the promotion of the blood flow, a condition of tonic contraction of the vessels is essential to the maintenance of the circulation. With all the vessels fully dilated the blood becomes contained in a system of flaccid tubes quite incapable of passing it on to the heart. The tonic contraction of the vessels adapts the vascular system to the amount of blood it contains, and by exercising pressure upon it, forces it forward, the direction being determined by the fact that the greatest amount of contraction and pressure is on the arterial side and the least on the venous, as well as by the presence of valves at the commencement of the arterial system and in the veins.

When we inquire into the measures by which this tonic contraction of the vessels is maintained, we again find an obvious answer to our inquiry in the nervous arrangements connected with the vascular system. The vessels, and especially the arterioles, are provided with two sets of nerve fibres, operating from a double set of nerve centres, the activities of which balance one another. The one set sends out impulses which increase the contraction of the musculature, the other set impulses which diminish it; these are the vaso-constrictors and vaso-dilators respectively. Their origin and path are for the most part different. The vaso-constrictors follow the ordinary course taken by motor nerves, except that they are distributed through the sympathetic; whereas most of the vaso dilators, as Dr. Bayliss has demonstrated, follow the course of the afferent nerve fibres and have their origin or distribution along with these, or may even be identical with them. Through these nerves the tone of the blood vessels, and thus the efficiency of the circulation, is maintained by the nervous system. The impulses which start from the nerve centres to produce vaso-constriction or vaso-dilatation originate, as in the case of those which affect the heart, mainly from the periphery—from the skin and other sense organs, viscera, muscles, heart, and even from the blood vessels themselves. The impulses therefore act in a reflex manner, causing either general or local effects according to the nature and situation of the part where they originate and of the particular afferent nerve terminations which are set in activity. But, as in the case of the heart, the several effects may also be produced by the action of higher centres upon the centres of origin of the vasomotor fibres.

In these various ways a balance is maintained in the vascular system whereby at a given time, and for a given set of conditions, the general or local blood pressure and the efficiency of the circulatory mechanism are normally regulated. Not very long ago the above would have been all that could have been postulated regarding the means by which the circulation is maintained in its efficiency. The work of Bernard and of Ludwig about the middle of the last century, and the innumerable researches which were set in activity by their discoveries, established beyond a doubt the important relations which subsist between the vascular and nervous systems, and especially the control which the nervous system exercises over the musculature of the heart and arteries. Certain facts were, however, not easily explained by this, such, for example, as the retention or recovery of some amount of tone after complete severance of parts of the vascular system from the central nervous system; although an attempt was made to explain these phenomena by the assumption of peripheral ganglia acting as reflex centres. Although it is not impossible that such centres may exist it is no longer necessary to assume their existence, since the phenomena they were required to account for may now be otherwise explained. For we know that certain glandular organs—which not long ago were regarded as of little importance and as to the functions of which nothing had been ascertained—yield on extraction materials which when introduced into the blood markedly influence both the rhythm and tone of

the heart and the contraction of the blood vessels. Those which produce the greatest effect of this nature are the medulla of the suprarenal capsules and the posterior lobe of the pituitary body. The action is not entirely similar in the two cases, for while an extract of suprarenal medulla increases both the rate of rhythm and the tone of cardiac muscle, and strongly stimulates to contraction the muscular tissue of the blood vessels (with the exception of those of the heart, and perhaps also of the lungs), an extract of the pituitary body, whilst markedly raising the tone of the cardiac muscle, does not increase its rate of rhythm, but may even diminish it, and while producing constriction of nearly all the arterioles in the body, tends to produce dilatation of those of the kidney, associated with an increased secretion of urine.

Although it has not yet been possible to prove in the case of the pituitary body that the substances which cause these effects are passed into the blood, and are thus normally brought to bear upon those tissues which are affected when the extracts are artificially introduced into the blood stream, such passage into the blood has been definitely proved to occur in the case of the suprarenal capsules. The blood flowing from these can, in fact, be shown to contain a larger amount of the pressor substance of the extract than ordinary blood. We are probably justified in concluding that a similar passage into the blood, either directly or indirectly (through lymph channels or by the cerebro-spinal fluid), occurs in the case of the pituitary body also. In both cases it is probable that the substances secreted exercise a material influence upon the circulatory system; this is, indeed, evidenced in the case of the suprarenals by the weakness of the heart and the low blood pressure which follow their destruction by disease or their experimental removal. And the fact that experimental removal of the pituitary body is equally fatal with that of the suprarenal capsules points to the supreme importance to the organism of the substances which are produced by it, the most active of these substances being those which affect the circulatory system.

The pituitary contains also a depressor substance which diminishes the force of the heart and inhibits the contraction of the arterioles, but the action of this is usually far overbalanced in the artificial extract by the material which produces the opposite effects. It is, however, possible that naturally the two substances may be passed into the blood in such proportions as the requirements of the vascular system and of the organism in general appear to indicate. If this is the case there would be a balancing action caused by the materials of the secretion comparable to that caused by the nerves which influence the contraction of the vascular musculature. It must further not be forgotten that a direct action upon the heart and blood vessels of hormones produced by these organs—perhaps also by other ductless glands—only represents a portion of their influence in increasing the efficiency of the circulation. It is probable that they also assist the nervous factors by their beneficial action upon the nerve centres themselves. In the instance of the pituitary body at least there appears to be evidence, mainly of a clinical character, that its extracts exert a tonic effect upon the nervous system in general. Such an effect is manifest in cases of shock, and may under normal circumstances be presumed to assist in maintaining condition of tonus of the nerve centres, which is essential to the performance of their functions, not the least important of which is, as we have seen, their action in maintaining the proper balance of the circulation.

In the above remarks I have dealt only, as I proposed at the start, with the circulation under normal conditions, and with such factors alone as are more strictly concerned with the vascular mechanism. I have not attempted to deal with the manner in which the circulation is affected by varying conditions of the blood, by variations in the general nutrition of the body, by drugs, or under abnormal conditions of the heart and vessels. I am content to leave the consideration of these to succeeding speakers, who will be better able than myself to discuss them satisfactorily.

DISCUSSION.

Dr. G. OLIVER (late of Harrogate) said: I think it will be admitted by clinicians that most of the leading factors of circulatory efficiency revealed by physiology are well supported by the results of medical observation, and in these days we can speak with much greater assurance on this point than we could do in the past, for now our instrumental methods enable us to follow in man himself some of the laboratory teaching on the physiology of the circulation. I shall touch briefly, and in a general way, on two or three practical aspects of the subject, such as these:

1. Cardio-vascular adjustment.
2. The vascular tone, its deficiency and excess.
3. The relation of peripheral resistance and blood pressure to circulatory efficiency.

1. *Cardio-vascular adjustment* to physiological work is a prominent feature of the efficient circulation. The obvious aim of the harmonious working of all parts of the circulation is to secure optimum efficiency with a minimum of stress on the heart and blood vessels. The adaptations existing within the circulatory apparatus are mainly effected by nerve controls. But I think it is possible that some self-adjustment may be furnished by the diastolic aortic blood pressure, which being proportionate to the peripheral resistance, may link up as it were the periphery to the ventricle, and enable the latter to nicely adjust the force of its contractions to the varying degrees of resistance ahead. The normal circulation as a mechanism thus ensures efficiency by being self adjusting. Hence a rise in the arterial pressure is modified by reduction in the ventricular output, by dilatation of vessel walls, and by filling up of the capacious veins; and a fall of that pressure is counteracted by an increased ventricular output, by a reduction of the calibre of the vessels, and by unloading of the veins.

It seems to me that the reserve capacity of the veins plays an important part in this adjustment, in providing for reduction or augmentation of the volume of blood on the arterial side of the circulation during the conditions of rest and work. All the adjusting factors in the efficient circulation work in harmony to shield the pump from undue stress in effort and to repair it during rest.

In the pathological field, in which the peripheral circulation is frequently thrown out of gear or is crippled, we apply this teaching of physiology to practical ends, when we prescribe resistance exercises, tension exercises, other forms of graduated gymnastics, and various kinds of baths. By these and similar means we endeavour to restore the physiological adjustment which has become impaired.

2. *The Vascular Tone.*—I shall quote some departures from normal arterial tone to illustrate the importance of the vasomotor mechanism in circulatory efficiency. Vascular tone varies considerably in different individuals, and in the same individual under various physiological conditions. It is raised by sleep, rest, good food, and a well-balanced metabolism; and is lowered by work, worry, strain, insufficient rest and sleep, and by whatever impairs the nutrition of the body generally and of the brain in particular.

For some twenty years or more I have found that the study of the effect produced on the pulse by change of posture affords useful guidance in estimating tone. In the earlier years I relied on the finger for appreciating the difference, if any, in the volume of the pulse in the erect posture and in recumbency. I found that there was not much appreciable difference when vascular tone was apparently normal; but the difference was quite marked between the smaller pulse of standing, and the voluminous one felt just as the patient assumed the recumbent position, whenever the system was weakened by any cause, such as haemorrhage, anaemia, asthenia, and especially exhaustion or weakness of the nervous system. Then it occurred to me that it might be possible to measure this difference by instrumental means; and the arteriometer was the outcome of that idea. Now we may go a step further in detecting this postural difference by using the manometer.

During the last year or two of my practice at Harrogate I frequently observed in patients in whom vasomotor control over gravity was diminished that the arterial pressure rose on the assumption of recumbency; but I found it necessary to make the observation without delay, as the pressure was sometimes apt to fall again sooner or later. In this way it can be shown that in normal subjects tonicity diminishes considerably under different physiological conditions, especially those of work and stress, whether

physical or mental; but the normal tone is quickly restored by rest, sleep, and food. These variations of slackened tone are but temporary.

It is quite otherwise, however, in various asthenic ailments and in conditions of chronic fag, of shock and worry; then the lowered vascular tone is not only more pronounced, but is continuous for some time, until, in fact, it is restored by prolonged rest or by rest judiciously alternated by exercise, by extra feeding, by guarded massage, and by vasomotor tonics. Over and over again have I proved the great practical importance of detecting this form of circulatory inefficiency by the postural test, and in successfully applying the appropriate physiological treatment; and repeatedly have I witnessed the perpetuation of it by the prescription of unlimited and unguarded exercise by those who were not cognizant of the inadequate state of the circulation.

I shall just mention that I have observed cases which suggest a probable causal relationship between splanchnic vasomotor inadequacy and syncope, vertigo, and even sudden lapses of consciousness (quasi-epilepsy); and I trust that other speakers will give us their views and experiences on this point, which is of some clinical importance.

Before the work of Leonard Hill in 1894 and 1895[1] there was no adequate explanation available of the postural variation observed in cases of asthenia, shock, and worry.[2] But that work furnished the all-sufficing interpretation, namely, impairment of the splanchnic vasomotor mechanism and diminished tone of the supporting muscles of the abdominal wall, which permit the influence of gravity to come into play in the erect posture.

Clinical observation also furnishes us with several varieties of excessive vascular tone. For examples, we have localized constrictions, such as those of Raynaud's disease and of the allied condition of persistent cold hands and feet; but these, as a rule, do not affect the circulation as a whole, and are generally the tell-tale of nervous disorder. Then there are the generalized constrictions of the faulty conditions of the blood produced by contracting kidney and chronic goutiness.

But besides such forms of persistent excess of arterial tone as these, we often meet with examples of increased contractility—I had almost said irritability—of the arteries, especially in the elderly, in whom we might anticipate same abeyance of vasomotor activity. Nevertheless, I think that a more susceptible state of the vasomotor mechanism may account for the rapid rise of the arterial pressure in many elderly subjects under psychic and other physiological influences. In fact, in the elderly generally I have sometimes thought that the range of the physiological variation of the arterial pressure is on the whole greater than in earlier periods of life. Possibly this increased contractility may in some cases be necessary to secure the requisite speed of the capillary blood flow in the declining years; and therefore it may be a factor which makes for efficiency in the circulation. In connexion with this question of hypertonic contractility of the arteries in the elderly I think that we ought to collate the pathological work of Savill on arterial hypermyotrophy in subjects over 40; for this subject is pathological rather than physiological. The teaching of these observations seems to me to point to the existence during life of a vicious circle of action and reaction between the central bundle of muscular fibres in the ventricle and the distributed muscular tissue in the vessels which maintains a supernormal arterial pressure and readily increases that pressure—the increment of pressure further developing the arterial hypermyotrophy.

3. *What is the Relation of the Arterial Blood Pressure to Efficiency in the Circulation?*—When viewed from the clinical standpoint this is a question of some importance; at the same time it is not one which can be easily answered in general terms applicable to all cases. It can only be satisfactorily solved in each case on its own merits. If we accept as the basis of circulatory efficiency a certain rate and pressure of capillary flow, which ensures adequate metabolism and the complete return of blood to the heart, then it is doubtful if we should regard arterial blood pressure as a necessary factor of circulatory efficiency; for both of these conditions may be secured, though the systolic pressure may have as wide a variation in different cases as from 100 to over 200 mm. Hg. We know that systolic pressures varying in the armlet readings from 100 to 150 mm. Hg are found in the average normal subject, and that such pressures are not only physiologically adequate, but are associated with a more stable form of circulatory efficiency than are the pressures either higher or lower than this range. Peripheral resistance is the most important fact in determining the arterial blood pressure; it may be so low that systolic pressures of from 90 to 100 may suffice, or it may be so great that much higher pressures, for example from 150 to over 200, are necessary. If in the former case we can raise, and in the latter case we can lower, the resistance, we shall probably make for well-being by increasing the stability and endurance of the circulation.

4. *Peripheral Resistance as a Factor in Circulatory Efficiency.*—I am disposed to draw two inferences from my observations on the circulation in man respecting the influence of peripheral resistance on the efficiency of the heart. One is the value of a certain degree of peripheral resistance in maintaining an efficient ventricle; and the other is the injurious effect of an excessive resistance, when sustained for some time, on the integrity of the ventricle as an adequate pump. Peripheral resistance reflexly stimulates the ventricle through nerve agency, and possibly by fluid conduction; and in doing so it should be an important factor in the nutrition of the heart muscle, which, like every other muscle, should grow with work. Would not the ventricular wall fail in nutrition and efficiency if we could considerably reduce the external resistance (the diastolic aortic pressure) say by one-third? for then the elastic recoil of the aorta, the coronary circulation and the stimulus to contraction would all be lessened. In normal man the resistance offered by the periphery of the circulation plays an important part in maintaining the nutrition and vigour of the ventricle—for him work is the best cardiac tonic.

In the pathological field we meet on the one hand with slack arteries and weak ventricles, and on the other with tightened-up vessels and obstructed peripheries with hypertrophied ventricles. In the latter class of cases the nutrition and vigour of the ventricle respond for a time to the supernormal resistance ahead, and this effect is salutary from the standpoint of an efficient circulation. But when the resistance cannot be relieved by treatment, the efficient life of the ventricle is shortened by the continued high grade of work it is called upon to perform.

Dr. LEONARD HILL (London) said: Physiologists have been in the habit of studying the circulation of the blood in animals which are anaesthetized, placed in the horizontal posture, and in a state of immobility. They have made artificial schemata and applied to the circulation the data obtained from these directly and often without justice. In the textbooks such statements are made as that the average blood pressure in all the large arteries is the same, and that the pressure in the veins varies from 10 mm. Hg to *nil*. Both statements properly pertain only to the horizontally placed and immobile animal. The changes due to the restless life of the animal, to alterations of posture, the ceaseless squeezing and churning movements of the muscles and viscera, have been left too much out of account, and the student, in consequence, gets no proper insight into the mechanism of the circulation.

The body must be regarded as a wet sponge enclosed in an elastic skin; the pores of the sponge are comparable in size to those of a clay filter, and the lining wall of the pores to soap films. The different parts of this protoplasmic sponge are squeezed vicariously by each muscular contraction, peristaltic or locomotor, and filled or emptied with the ceaseless change of posture which occurs in active life. It is the function of the heart to impel the blood through the arteries to all parts of the sponge, and of the muscles to squeeze the blood out of the sponge and back to the heart through the veins. The venous valves and the anatomical arrangement of the vessels makes the squeezing mechanism an effective one. The rhythmic impulsion to and fro seen in the "veins" of the slime fungi—myxomycetes—the extraordinary flow of water from the cut stems of vines, point to the potent influence of

[1] The Influence of Gravity on the Circulation, *Proc. Roy. Soc.*, and subsequent papers.
[2] *Pulse-gauging*, 1895, pp. 32 and 156 to 164.

tissue metabolism as a factor in promoting and controlling capillary flow. The observation of the capillary circulation is made habitually in transparent membranes, which have a comparatively rigid structure and are fixed in an immobile state; it can give little insight into the nature of the capillary flow in such organs as the liver or spleen. In the liver the varying turgor of the cells and the rhythmic squeezing of the respiratory muscles have a profound effect.

During intestinal absorption the muscle cells of the villi and the varying turgor of the lining epithelium together impel both the flow of blood in the capillary net and of lymph in the lacteals. The gut blanches during peristaltic contraction, and flushes red again on relaxation, and the proper exercise of its motions not only furthers the contents, but pumps the blood through and prevents venous congestion. The respiratory movements act as a pump to the whole of the abdominal organs, squeezed as they are between the diaphragm, the levator ani and the muscles of the abdominal wall.

The outpouring of sweat in a Turkish bath, of urine when diuresis is at its height after drinking much water, must influence the capillary flow very greatly. It is as if fresh outlets were opened and the peripheral resistance is lowered not only by the concomitant vaso-dilatation, but by the escape of water through the gland ducts. The pulsatile expansion of the organs furthers the flow. The whole organ expands, becomes turgid on systole and shrinks on diastole. The living cells separate and transfer the secretion from capillary to ductule. The fluid is moved on both in capillary and ductule by the rhythmic expansion and shrinkage of the organ. The pressure in such an organ as the brain or kidney is the same throughout the organ and is equal to the capillary venous pressure. The tissue fluid, cerebro-spinal fluid, lymph and urine is at the same pressure as the capillary venous pressure. The conditions are such that there can be no filtration pressure as is usually supposed. Wet films of protoplasm form the basis of structure and the transference of liquid is by the secretory power of the living cells, at bottom a physical chemical process of the same type as that seen in the formation and extrusion of vacuoles by protozoa.

In the performance of great muscular efforts the muscles of the strong man become hard as iron. Consider the blows which a professional boxer can bear on his abdominal muscles, the squeezes of wrestling giants, the rhythmic squeezes and relaxations of the leg muscles of the runner or bicyclist. The circulation must be so arranged that blood can either be squeezed out of a part without undue congestion of other parts, or held within the compressed and contracted part, the fluid in this case transmitting the pressure equally and in all directions. In the contracting heart and clinched fist we see the blood squeezed out of the capillaries and the pumping action of the muscles demonstrated. In great running efforts the muscles are exsanguined to such an extent that lactic acid is produced, owing to want of oxygen. The pressure, both in the arteries and veins, is high in the forearm when held dependent, while it becomes *nil* in the capillaries immediately after clenching the fist. The capillaries are blanched, and until the sponge refills we have the paradoxical condition of a high pressure in the artery that feeds and the vein that drains the part and no positive pressure in the capillaries. This is a constantly occurring condition of the circulation, and depends on the squeezing effect of muscular movement and the valves in the veins.

I have measured the capillary pressure in man by inserting a hollow needle into the limb or organ; the needle filled with water is connected to a glass tube which contains an air bubble as an index, and this is connected in its turn to a manometer and a syringe. The pressure is raised in this system until the air bubble moves inwards—that is, until the capillary pressure is overcome. Such measurements of the fluid pressure as I have made in cases of oedema and hydrocephalus show a positive pressure of some 50–60 mm. H_2O. When the oedematous limb or hydrocephalus is lifted up the pressure does not fall, but rises, owing to the squeezing effect of the hands that lift it. The pressure pertains equally throughout the limb or skull cavity. In such a case the capillary venous pressure must just exceed the fluid pressure for otherwise the circulation would not take place. The fluid pressure is circulatory in origin, as is shown by the immediate disappearance on death of any positive pressure other than that due to the hydrostatic effect of gravity. On tapping the hydrocephalus the pressure falls to atmospheric pressure, but rises again after two or three days as the choroidal fringes secrete the fluid and once more distend the part. It is the secretory activity of the cells which regulates the pressure, and the pressure in the capillaries must keep pace with that.

In states of metabolic activity, in inflammation, the tissue turgescence and capillary pressure increase together. The tense state of the inflamed part is maintained by the heart beat, but the metabolic changes in the part initiate and entail the increased vascular pressure. In an encapsulated organ such as the marrow, brain or kidney, an increase in the amount of tissue fluid and turgor of cells diminishes the volume of blood in the vessels, while if the fluid pass from the tissues into the blood vessels these dilate and contain more blood. Thus in the infective states bone marrow may exhibit congested vessels with shrunken marrow cells, wasted fat cells resulting from the migration of cell substance from marrow into blood. If there is more tissue fluid in a rigidly encapsulated organ there must be less blood; the blood vessels will be narrower, the capillary venous pressure higher and the blood flow faster. The conditions vary from congested blood vessels, stagnant flow and shrunken cells, to that of taut blood vessels, rapid flow and turgid cells. A sudden increase of arterial pressure does not alter the volume of blood in an encapsulated organ but increases the rate of flow through it. The relative volumes of blood and tissue fluid depend on metabolic changes which are comparatively slow. The capsule of the organ and the skin are parts of no little importance in the mechanism of the circulation. Expansion of the capsule gives room for more blood and more tissue fluid. Hence the relief obtained on opening the skull or incising the skin in pathological states.

The current doctrines of pathology are pervaded with ideas of back pressure, of the transudation of liquid filtered from the capillaries under pressure. There is no evidence that back pressure really exists. On obstructing the flow in the veins the venous pressure rises for a brief period, but quickly goes back to normal. Oedema, dropsy, etc, are the result not of increased capillary pressure and filtration but of deficient blood flow and altered nutrition of the tissue cells.

The man in good condition, like the wild animal, is "hard," with muscles like iron, blood vessels tightened up, a rapid blood flow, little fluid in the tissue spaces, a maximum turgidity of cells, no superfluous fat, and a taut skin. His heart is adjusted in size and power to the needs of his muscles. Observations I have made with the Roentgen rays afford no evidence that the heart is dilated by severe muscular work. The heart appears smaller during the subsequent stage of dyspnœa. Muscular exercise and exposure to cool winds are the causes of metabolic change. Muscular exercise may increase the CO_2 output, the oxygen use, and the rate of blood flow six and even ten times the resting value. Hot, moist atmospheres congest the skin, lessen muscular and metabolic activity, and slacken the cerebral circulation. Studying the effects of very hot baths on man, I have watched the systolic pressure fall to 60 mm. Hg and the pulse-rate rise to 150. Faintness came on when the vertical posture was assumed; all the symptoms were immediately relieved by a cold douche which constricted the cutaneous vessels, even though the rectal temperature remained unchanged at, say, 103° F. I find, when shut in a closed respiratory chamber until the CO_2 percentage reaches 4.5 per cent. and the wet bulb temperature 85° F., an electric fan which simply stirs the stale, warm air affords the greatest relief because it cools the skin and relieves the heat-regulating mechanism.

The vasomotor system regulates the flow in organs in answer to their metabolic activity, maintains a fairly constant aortic pressure under varying conditions of cardiac output, hangs out the signs of sex, regulates heat loss, and lastly, controls the circulation during change of posture.

We have measured the systolic blood pressure in man in the horizontal, standing, and inverted positions. Students have lent themselves to being suspended by the heels. The figures show that the pressure in the brachial artery

is maintained at about the same level in all three postures, while that in the tibial arteries varies very widely. The influence of gravity is compensated for in the inverted posture by the lessened output of heart and dilatation in the splanchnic area, and so perfectly that the pressure in the brachial artery is no higher in the inverted posture than it is in the standing position. And yet the column of blood which divides the arm and leg arteries is equal to some 70 mm. Hg. The nervous mechanism which thus keeps the pressure constant in the ascending aorta must act continually, and demands study in pathological states.

The usual method of recording the blood pressure in man is by observing the obliteration of the pulse. The observations we have made show us that the conductance of the crest of the pulse wave depends upon the arterial wall. A soft artery with open peripheral field will not conduct the wave as well as a more contracted and rigid artery. Local heat can be used to soften an artery and dilate the peripheral field and demonstrate this fact. In all cases of aortic regurgitation we find the systolic pressure in the tibial arteries much higher than in the brachial arteries. The leg arteries in these cases are kept more taut, and the systolic crest is a very high one. We have been able to lessen the difference in these aortic cases by placing the feet in hot water, but only to a limited extent. We have not been able to alter the difference by placing the leg in bent postures, so the difference does not depend on the straighter pathway down the leg arteries. The cause of the difference must be mainly in the conductance of the wave along the aorta. Inequality of pressures due to aneurysm of the arch of the aorta are caused by the soft sac lessening the conductance of the crest.

Sir James Barr (Liverpool) said: Professor Schäfer has wisely emphasized the important part played by the nervous system in maintaining an efficient circulation; this is of paramount importance in the present day, when the influence of the nervous system is neglected in favour of some popular but vague and nebulous ideas on the myogenic function of the cardiac muscle. There is no one likely to overlook the important functions of the cardiac muscle, no matter how exalted an opinion he may hold of the controlling influence of the nervous system. The healthy nutrition and function of the heart muscle are dominated by the nervous system, and the tone and irritability of the muscle enables it to respond to stimuli directly without any necessity for such stimuli being transmitted through the nervous system. In the case of the knee-jerk the maintenance of the reflex arc is necessary, but the response is much quicker than could be attained through a nerve reflex. So with a healthy cardiac muscle—the irritability is so great that it responds at once and directly, except during the refractory period, to the variations in the diastolic pressure within the heart.

It is not sufficiently recognized that the two sides of the heart are independent organs linked together for mutual assistance and support, but each side has got its own work to do, and it does it to a certain extent, independently of the other. The right side of the heart is more sensitive than the left, and its rhythmic contraction ordinarily responds to less than an atmosphere of intracardiac pressure, while the left side requires rather more than an atmosphere to start an efficient contraction. The effect of this is that the systolic contraction is almost invariably started in the right side, and when you get doubling of the first sound—which in a healthy heart occurs at the beginning of every deep inspiration—the first element is associated with the closure and tension of the tricuspid valve. If the systolic contraction were due to any myogenic stimulus travelling along the bundle of His, then both healthy ventricles should start their contraction absolutely synchronously; but there is no more common event in cardiac physiology than a slight dissociation in the beginning and ending of the two ventricular systoles.

If a patient faints, do not wait for the sino-auricular node to start ticking again, and do not try any artificial respiration, but simply hang the patient up by the heels; in this way you raise the diastolic pressure within the right auricle and ventricle, which starts their contraction, and the left side soon follows suit. Many a case of sudden death, I am convinced, might thus be averted if any sensible person were at hand to carry out this simple procedure. On the other hand, under such circumstances the popular imaginary myogenic stimulus which is supposed to initiate cardiac contraction leads nowhere but to the grave. Increased diastolic pressure within the heart leads to dilatation, while increased resistance in front induces cardiac hypertrophy. Chronic diseases of the heart most frequently arise from causes acting on the periphery.

The act of respiration renders great assistance to the circulation, especially to the right side of the heart. I have recorded a case of so-called heart-block where we could at any time stop the pulse and bring on an epileptic seizure by simply making the patient take a deep breath and hold his chest fully expanded. Here the capacious lungs acted as a reservoir, and when the left side had emptied itself a sufficient amount of blood did not reach it to stimulate contraction until the feeble right side had raised the pressure in the pulmonary circuit. Both in the Valsalva and Müller experiments the size of the heart is very materially diminished, and if the former experiment be efficiently carried out, the entrance of blood to the chest can be so effectually shut as to cause the disappearance of the pulse as long as the person is able to hold his breath, and the heart becomes smaller and smaller until at last it is not more than half its former size.

For an efficient circulation the maintenance of a healthy elastic aorta is absolutely essential. In my opinion, physiologists have placed too low an estimate on the velocity of the blood in the aorta when they put it down as 320 mm. in the second. They have arrived at this conclusion by a process of guess-work from rather crude experiments on animals, which are not at all applicable to the human subject. In my opinion, the velocity in any individual can be fairly accurately calculated from the fall in the pressure gradient. For an efficient circulation you should have a low systolic, and a relatively high diastolic, pressure; this gives you a slow but steady velocity, and does not strain the elastic coats of the aorta. As the blood is leaving the heart its energy is kinetic, the greater portion of which is immediately stored up as potential in the elastic coats of the aorta and pulmonary artery, and again given out as kinetic energy during the contraction of the vessels. The diastolic pressure in the great vessels fairly represents the resistance to the outflow from the heart, while the difference between the diastolic and systolic pressures is the portion of the cardiac energy which is not stored up in the elastic walls of the vessels, but used as the effective head of the liquid in the production of velocity. If there be no great disparity between the systolic and diastolic pressures then the velocity is comparatively slow and fairly uniform during the whole cardiac cycle, though of course rather quicker during the systolic period. For an effective circulation the arterial diastolic pressure should always be sufficient, and more than sufficient, to overcome the resistance of the arterioles, the capillaries, and the viscosity of the blood. If the diastolic pressure were not sufficient to overcome the resistance, the circulation of the main arteries would come to a standstill towards the end of the diastolic period of the heart. The way to lessen the work of the heart is to reduce the peripheral resistance.

As the arterial velocity depends on the energy or effective head minus the resistance to the outflow (in the aorta the viscosity of the blood can be left out of account), so when there is a great disparity between the systolic and diastolic pressures, such as occurs in free aortic regurgitation, there is defective storage and the velocity becomes very great, especially during the cardiac systole. In my Toronto address I showed that when the difference between the systolic and diastolic pressures exceeds 40 mm. of mercury the elasticity of the aorta is becoming impaired, and it is time to think about repairs. In cases of chronic granular kidneys where the arteries are contracted, and the systolic and diastolic pressures very high, you do not get tortuous arteries; but when the arteries are dilated, and there is a great difference between the systolic and diastolic pressures you get longitudinal straining of the arteries, the greater curve of the arch of the aorta gets dilated, and the muscular arteries become tortuous.

The importance of pressure and velocity in the aorta will be readily apparent from the following calculations: A fall in the pressure gradient in the aorta from 100 to

80 mm. of mercury gives you a theoretical velocity per second of 525 mm. of blood (I add blood because one gentleman thought I meant to reckon the velocity in millimetres of mercury, which, by the way, does not exist in the aorta). This is a very easy going circulation, which would scarcely suit the hurry and bustle of the present day, but it is such a circulation as was probably possessed by Methuselah. With a pressure of 150 mm., and a resistance of 120 mm. of mercury, you get a blood velocity of 640 mm. in the second. This is the kind of circulation which would suit a professor or anyone who had no undue physical or mental strain; it might carry him well into the eighties if he were not foolish enough to get killed in some other way. Now a fall in the pressure gradient from 150 to 100 mm. of mercury gives you a blood velocity of 1,120 mm. per second; by this time the elasticity of the aorta has been seriously damaged, and its repair should not be entrusted to an amateur. With a systolic pressure of 250 mm. and a resistance of 120 mm. of mercury, which often occurs in cases of free aortic regurgitation, you would have a velocity of the blood at the rate of 2,450 mm. in the second, which would chiefly occur during the cardiac systole. This gives rise to a marked recoil of the heart at the end of the systole, and to positive and negative waves in the circulation which obstruct one another. When failure begins to set in you may find the force of this big powerful organ, which may shake the whole chest, and even the bed in which the patient is lying, only poorly represented at the periphery. In such cases the diastolic pressure in the left ventricle is great, this cavity is dilated, the elasticity of the aorta is seriously impaired, and storage of the energy in its wall is very defective, the pressure and the velocity are more or less intermittent, there is an enormous waste of energy, and failure is bound to occur rapidly and perhaps suddenly. In the foregoing calculations, mercury, which is 13.6 times heavier than water, has been reckoned as thirteen times heavier than blood.

Professor Schäfer has told us of two secretions—the suprarenal and the pituitary—which are essential to life. I would like to add the importance of the lime salts in the maintenance of an efficient circulation. The presence of calcium ions is necessary for all effective muscular contraction; all the soluble lime salts increase the viscosity of the blood and lessen its ready transudation through the capillary walls. The arterioles form the first line of resistance to the outflow from the arteries, and perhaps are the main factor in maintaining the arterial pressure; but I am now convinced that the viscosity of the blood in the capillaries is an important element in maintaining high arterial pressure, and one of the most important causes of this viscosity is calcium. This is not readily appreciated, as none of the lime salts quickly raise the arterial pressure. Calcium is a slowly acting drug; it is absorbed with difficulty, and the blood can only hold a limited quantity; moreover the resistance from viscosity is a variable quantity and takes time to develop cardiac hypertrophy. On the other hand, by the free use of decalcifying agents you lessen the viscosity of the blood and rapidly lower the arterial blood pressure; in this way you get indirect evidence of the effects of calcium in raising arterial blood pressure. Since Blair Bell has given us a reliable method of estimating the lime salts in the blood there is no reason for any one remaining ignorant on this important subject.

The capillary circulation is chiefly carried on by the energy derived from the blood in the arterioles and arteries, and to a less and more variable extent by muscular contraction. I have shown that physiologists have underestimated the varying velocity of the blood in the aorta, so I believe they have very much underestimated the velocity in the capillaries, which they set down at 0.5 mm. per second. Their conclusion has been drawn from observations on the frog, which is scarcely comparable with man, and Vierordt reckoned the velocity in the retinal capillaries, a very difficult method of observation, and, moreover, if he had inverted the subject he would probably have found that the velocity was so great as not to be determinable by this method. I estimate the capillary velocity by compressing the blood out of a flat network of capillaries, say the back of the finger or hand, and then time with a stop-watch recording fifths of a second the period it takes the blood to return. You must select a spot where the blood returns equally from all parts of the circumference of the area which has been compressed.

If you divide the radius of the compressed area by the return time of the blood you get the velocity per second. I usually employ a rod 10 mm. in diameter, the radius of which is 5, but as the red cells travel in the axis of the capillaries, and occupy about four-fifths of their lumen, I reckon the actual to be about four-fifths of the observed velocity. Therefore, when you use the rod, if you divide 4 mm. by the time of the return you get approximately the actual capillary velocity. As the capillary velocity is very much influenced by the arterial pressure I always make my observations on the hand or finger held at the level of the heart. If you lower the finger about 320 mm. you double the capillary velocity. You can make a similar observation on the web of a frog's foot; when you are examining the capillary circulation under the microscope you can visibly increase the velocity of the blood by raising the body of the frog ½ in. or 1 in. This does not add a great head to the liquid, but it is quite sufficient to so much increase the velocity that the arterioles are not able to keep the open capillary bed full, and then the blood commences to run backwards in many of the capillaries.

If the velocity of the blood in the capillaries of a warm hand at the level of the heart be less than 4 mm. in a second as a rule there is too much lime in the blood, or the heart's action may be very feeble. When the capillary circulation is less than 1 mm. in the second the blood gets surcharged with CO_2—witness the hand on a cold day or the liver under ordinary circumstances. When the velocity at the level of the heart falls to the physiological standard of 0.2 to 0.5 mm. per second you can take up the refrain, "the hour of my departure's come."

The pressure in the systemic veins depends on three factors: (1) The obstruction to the inflow to the chest; (2) the hydrostatic effect of the column of blood; and (3) the energy transmitted through the capillaries. The velocity depends on the *vis a tergo*, and varies enormously. Here the fall in the pressure gradient from the periphery to the centre is the determining factor. The velocity is calculated by emptying a long piece of vein between two valves and then timing with a stop-watch the period it takes the blood to fill the empty vein. I have seen 23 cm. of a vein in a dependent arm filled in 0.2 of a second, or a velocity of 115 cm. in a second, a velocity greater than ordinarily occurs in the aorta. I claim to have been the first to discover the fact that venous blood runs more quickly up the hill than it does down it; in a vein of the arm the velocity is sometimes five times greater when the arm is hanging than when it is held horizontally at the level of the shoulder. The total amount of blood passing through a limb in a given time may be two or three times greater when the limb is dependent than when it is elevated. Muscular contraction is also an important agent in hurrying on the venous blood. The foregoing observations show how kinetic and potential energy in every part of the circulation are constantly varying, and the cocksure methods of physiologists require considerable revision.

Dr. ALEXANDER HAIG (London) said: Among blood conditions that affect the circulation, by far the most important is the quantity of uric acid to be found at any given moment in the circulating fluids. I have been making a similar statement in my writings for some eighteen years, and meanwhile facts tending to prove its truth have been steadily accumulating. I will now mention a few main points.

My early investigations on the causation of migraine showed that it was accompanied by a large excretion of uric acid in the urine, and high blood pressure was the apparent cause of the pain. I afterwards made similar observations in many cases of epilepsy. I then found that the normal daily fluctuations in the excretion of uric acid were accompanied by corresponding fluctuations of blood pressure, and that drugs which increase excretion raise blood pressure, while those that diminish excretion lower blood pressure. I could not only alter the amount of uric acid in blood and urine at pleasure, but found that I could alter and control the blood pressure at the same time.

Then following the observation of Raynaud, I began to measure accurately the rate of the capillary circulation

(having previously used the fatigue image in the eye for the same purpose), and finally invented an instrument and method for measuring capillary circulation in the skin, which I still use. I thus found that the capillary circulation was slow in all conditions in which my previous investigations, and those of others, had demonstrated the presence of excess of uric acid in the blood and urine. Further, I could control by (1) diet, (2) drugs, (3) heat or cold, the quantity of uric acid in both blood and urine, and I controlled at the same time both capillary circulation and blood pressure.

Thus, step by step, I was led to conclude that uric acid had a great influence on capillary circulation and blood pressure, which always fluctuated with it in physiology, pathology, and drug action. Among drugs the salicylates gave the most marked results, for large doses quickly doubled the uric acid in the blood with slowing of capillary circulation and rise of blood pressure, or, on the other hand, by the retentive action of small doses,[1] the blood could be almost cleared of uric acid with quickening of capillary circulation and fall of blood pressure.

It would take too long even to name the drugs which produce parallel effects, and many the action of which can be very simply explained by their influence on the solubility of uric acid in the blood.

Thus, in all directions and by many methods was it proved that the rate of the capillary circulation was a measure of the quantity of uric acid in the blood, and the only possible inference from this fact was that uric acid controlled the capillary circulation. And it does this, not only in relation to pathology and drug action, but in physiology and throughout life.

A patient has high blood pressure, you can lower it by any means that will clear the blood, and at the same time the urine, from excess of uric acid. Your results are rendered visible by the rate of the capillary circulation. Further, there is a definite ratio between the rate of the capillary circulation and the mean blood pressure, which is that the capillary circulation rate, measured in half-seconds, multiplied by twenty, gives the corresponding blood pressure, if the heart muscle is normal. If the heart is hypertrophied, the blood pressure may be above this ratio; if the heart muscle is weak or fatty, blood pressure will be below the ratio, and the amount by which it falls below it is a measure of the heart weakness.

The quantity of uric acid in the blood can be easily measured by the Barker-Smith process, and for its clinical estimation I must refer to my books, especially *Uric Acid in the Clinic* (Churchill, London, 1910), where it will be seen that the capillary circulation is a reliable guide to its quantity.

A single dose of uric acid taken by mouth can be watched as it passes through the blood by means of half-hourly observations of the capillary circulation. It will often be found to pass through it in the next alkaline tide after its administration, and if the circulation results are thrown into curves there will be a quite visible slowing and rise of blood pressure at the time of its passage.

I do not for a moment doubt the effect of the nerves on the tone of the heart and vessels, but this is more or less constant and does not vary, as do uric acid and its effects, with the daily and hourly fluctuations in the solvent powers of the blood for uric acid and its salts.

Thus in every direction is capillary circulation a measure of the quantity of uric acid present in the blood, and this, I submit, could not be the case if the capillary circulation was chiefly controlled by the nervous system, apart from uric acid. I conclude from these and many other facts which there is no time to mention, that the high blood pressure so commonly met with to-day is the result of the numerous uric-acid compounds in our foods and drinks.

Dr. Buckmaster (London) mentioned that in certain experiments where the perfusion of the living lungs was carried out with hirudinized blood of the same animal, in some cases, notably the cat, subsidence of the red blood corpuscles occurred with such ease that, quite apart from the question of the viscosity of the blood, this mechanical separation of the morphological constituents from the plasma might well be a factor that impedes the circulation in any part of the body. Notwithstanding the rate of the flow through the lungs, this subsidence of corpuscles takes place with such readiness that within a few minutes from commencing the experiment the transfusion becomes impossible—that is, an oedema of the lungs occurs and all that can be driven through the lung resembles mud in its consistence.

Dr. Jolly (Edinburgh) described some of the newer methods of investigating the action of the heart by means of the string galvanometer.

Professor Schäfer briefly replied to some of the points which had been raised in the course of the discussion.

[1] See *Proceedings of the Royal Society of Medicine*, February, 1909.

DISCUSSION ON FOOD REQUIREMENTS FOR SUSTENANCE AND WORK.

OPENING PAPER.

By Lieutenant-Colonel Melville, R.A.M.C.

In opening the discussion on the question of "Food Requirements for Sustenance and Work," I propose to take an actual concrete instance of men doing a measured amount of work on a measured amount of food, in which the effect of these two factors on the weight, general condition, and well-being of the individuals experimented on was carefully recorded.

As regards the men who formed the subject of the experiment, these were twenty infantry soldiers of ages between 18 and 38. No special selection as to habits or physique was made, a general examination to exclude any actually unsound man only being carried out. There was also a considerable range of physique, the heaviest man being, at starting, 76·6 kilos (12 st.), and the lightest 55.30 kilos (8 st. 9lb.). The average weight of the party was 64.15 kilos (10 st.). The work done was a daily march averaging between twelve and thirteen miles, carrying a load of 24.55 kilos (54 lb.). This march was executed for six days continuously, then one day's rest intervened before the completion of the experiment by five more days' marching. The external work thus performed was calculated on a formula adapted from Zuntz's work[1] as follows: For every kilo of body weight transported 1 metre horizontally at the rate of 94 metres to the minute (the actual rate practised) 0.0006 calories were expended. For every kilo raised through 1 metre vertical 0.0075 calories were expended. The distances marched and the vertical distances ascended were carefully measured on a 6-in. ordnance map. The horizontal and vertical works were multiplied separately by the average weight of the loaded man, and the sum of these taken to represent the day's work on the march. The average daily amount of energy thus expended came (after various corrections on account of individual men who fell out, or who remained in camp on certain days) to 1,034 calories—that is, roughly, 90 calories per mile of ordinary give-and-take road. It may be noted that this figure has been found to be fairly constant for the South of England, where long or steep hills are the exception, for a man of the average weight, and carrying the average load mentioned. To this a certain amount had to be added to account for internal work and for external work during non-marching hours. This was assumed at 3,000 calories. The grounds for this assumption may be briefly stated. Zuntz gives the expenditure of a man at absolute rest as 1,700 calories, which is increased by 30 to 50 per cent. if a purely sedentary life—confined to one room, reading and writing—be led. This gives us a figure of 2,200 to 2,250 calories. The men of the experiment during the ten non-marching, waking hours (allowing ten for sleep and four for marching) led a distinctly active life. There was, of course, the ordinary work of camp life to be done (on one occasion the camp had to be struck and moved by hand a quarter of a mile to a less exposed site), and a good deal of football and quoits were played. If we estimate this as equivalent to one mile for each working hour in excess of sedentary life, this will give us 800 calories—that is, 3,000, taking the lowest sedentary figure arrived at by Zuntz. I note, since the above was first written, that the same observer places the energy expended by the infantry soldier on a day of rest at the same figure of 3,000

calories.[2] The total work, internal and external, done per diem amounted, therefore, to 4,034 calories.

The food supplied varied somewhat during the first and second periods of the experiment. It consisted essentially of bread, meat, potatoes, jam, and sugar, with tea, pepper and salt, etc. During the first six days tinned meat and biscuit were issued; during the remainder of the period fresh meat and bread. The ultimate constituent principles were as follows: During the first week, proteins, 190 grams; carbohydrates, 510 grams; and fats, 58 grams. During the remainder of the time: proteins, 145 grams; carbohydrates, 450 grams; and fats, 110 grams. The total energy value averaged on the first week 3,426 calories, and on the second 3,503 calories. The averages over the whole period were as follows: Proteins, 168 grams; carbohydrates, 480 grams; and fats, 84 grams. The average gross daily energy supplied was 3,481 calories. From this 10 per cent. should be deducted for unavoidable waste, giving net, say, 3,140 calories.

(It must be noted here that in the above calculation the energy value of a certain amount of alcohol has been included; 2½ oz. of whisky on one occasion, and of rum on another, were issued, owing to weather conditions and extra fatigue.)

Comparing the amount of energy supplied by the food with that expended in work, taking the average per man per day in each case, we find that the former fell short of the latter by about 890 calories.

We now come to the combined effect of the work and food on the men. There was a general and steady rise in weight over the first three days of the experiment, averaging 0.55 kilos, followed by a period of two days during which the weight remained stationary. After this, with the exception of a slight rise the day after the day of rest, the weight fell steadily till the end of the experiment. The average fall was 1·22 kilo for the maximum, and 1 kilo over the whole period. These same features of an initial rise, followed by a subsequent fall, were observed in the case of the officers accompanying the party. These were restricted in food in the same way as the men, but carried slightly less weight; being all older men, and chiefly heavier than the main party, their figures have not been included with the rest, but are useful for purposes of comparison. This initial rise is of considerable interest. It could not be due to an increase in stored fat, since in the earlier days this constituent was markedly lacking in the diet. The most probable explanation is that it was due to retention of water in the tissue. There seems some evidence to prove that a greater amount of fluid was consumed, by some of the party at least, in the earlier days, when corned meat was eaten, than they had been in the habit of drinking prior to the march. This was not universally the case, however, and it is doubtful whether the increased ingestion due to thirst produced by salt meat would not be more than balanced by an increased diuresis produced by the action of the potassium nitrate to which the thirst was attributable. In any case this explanation does not seem sufficient to account for the steady rise that continued for three days, and whose effects persisted till the end of the first phase of the experiment. It is to be noted that the amount of protein ingested per day during the first six days was 190 grams, a distinctly high figure, amounting to a rise of probably 30 per cent. on the amount ordinarily consumed in barracks. At the same time the carbohydrate allowance was increased by about 25 per cent. A storage of protein in the body does not therefore appear improbable, in view of the stimulus to muscle growth afforded by the regular work demanded of the men during marching. Human muscle consisting of about 20 per cent. protein and 80 per cent. water, if we assume that the excess of protein above the customary barrack allowance was retained, this being say 50 grams, the total increase in weight obtained in this manner would if continued over the first three days amount to 750 grams. Since the actual increase averaged 550 grams, this would allow for a certain excess expenditure of fat and glycogen in the production of energy. It must be noted, too, that during the first six days of the experiment there was a distinct increase in calf circumference of, on an average, 0.22 of an inch. The average abdominal girth also increased by 0.54 of an inch, possibly due to increase in the lumbar muscles. Whatever the explanation this point of the initial increase in weight seems a suggestive one for discussion and future investigation. During the last days of the experiment the reduction in weight was marked and continuous. During the last five days the average loss was 234 grams per diem, or just over half a pound. This loss must have been either in fat or flesh, or partly in one and partly in the other. If in fat alone the daily loss of energy would have been $234 \times 9\,3 = 2{,}176$ calories, whereas the estimated daily loss was, over these particular days, under 890 calories. It must also be remembered that during the earlier part of the experiment the ration of fat had been extremely low, only 58 grams. There was, therefore, probably very little superfluous fat available in the bodies concerned in the experiment. It must be added that already by this time there was a generally sunk appearance in the faces of the men, attributable to loss of adipose tissue. If we refer to an experiment lately recorded by Benedict, and quoted by Zuntz,[3] we find that in a seven days' starvation experiment the daily loss of weight was 526 grams, this being represented by a loss of 69.5 grams protein, 139.6 grams fat, and 23 grams glycogen. The protein loss corresponded to a loss of 347 grams of flesh. The total loss of energy (measured by the calorimeter) was 1,696 calories. These figures give a loss of energy of 1,298 calories from destruction of fat, 285 calories from protein, and 94 calories from glycogen. Thus, whilst the greater part of the lacking energy was found from fat, a correspondingly high percentage of the loss of weight was due to waste of fleshy tissues. If we assume that the loss of weight of the men under observation was similarly distributed, we shall find that they should have lost in energy $\frac{234}{525} \times 1{,}696$, that is 756 calories, whereas, as a matter of fact, the calculated deficiency of food as compared with work done was, as already stated, less than 890.

The agreement is fairly close, and we shall probably be justified in concluding that the daily loss of weight was represented by a loss of about 60 grams of fat and 170 grams of flesh. The officers were, as a whole, losing weight about half as fast again as the men, but as already noted they were on the average heavier individuals. The heaviest man in the party lost more rapidly even than the officers. The measurements taken at the end of the experiment showed a distinct wastage. This was most marked in the chest, which fell on the average 0.56 in. in the first week, 0.42 in. in the second week, and 0.98 in. on the whole period. There can be little doubt that this was attributable to wastage of pectoral muscle. In the second week the abdominal girth fell over 1 in., whilst the circumference of the calf fell 0.16 in. Too much reliance cannot be placed on these figures as showing actual quantity of loss, but they have a distinct value as demonstrating the fact that loss occurred, especially when taken together with the loss of weight.

Turning now to the question of the different constituents of the diet. In the first six days the average amount of protein consumed was, as already stated, 190 grams, of which 100 grams was furnished in the form of corned beef, and 75 grams in biscuit. I have no doubt in my own mind that this allowance is ample, and if it errs does so on the side of generosity. A substitution of a certain amount of fat for some of the protein would undoubtedly have been an advantage. In the second week the average amount of protein was 145 grams, supplied in the form of fresh meat and bread. I have no hesitation in saying that this allowance is as low as it is advisable to go, and might well be increased, especially when hard work is demanded of men under conditions of exposure. This is, I am aware, in contradiction to the theory held by many at the present day. I would, however, submit that experiments carried out on men leading sheltered lives, with regular meals, and rest, are no safe guide when we are dealing with the food of men exposed to even the slight hardship of camp life in bad weather (I may remark that throughout the experiment in question the weather was atrocious, and, speaking for myself, I never slept in dry bedding or put on dry underclothing after the first day), and much less so when we are dealing with the inevitable vicissitudes of active service. I need hardly remind you of the fact that the Japanese have been compelled to increase their protein ration in the navy to 150 grams, and have found marked benefit to result

therefrom. As to any possible harm resulting from a high protein diet, speaking here of men leading active animal lives, I must confess I have failed yet to see any positive proof of the fact. I think we are too apt to rely upon isolated experiments as against prolonged experience. The British inhabitants of these inlands have for many hundred years been large meat eaters. Taking the labouring class, certainly we may say that the only check to meat eating has been poverty. When the Englishman can afford meat he will have it; I think I am safe in saying that the enormous majority of my audience, as indeed of any other assembly in the country, are descended from a large protein eating stock, and, when they are able to afford it, a meat eating stock, and I confess I fail to see any marked signs of that degeneracy which, if all the evil tales we hear were true, should be written at large on the bodies and faces of my audience.

Far more important to my mind than the actual quantity of protein is its nature, that is to say, the foodstuffs in which it is furnished. Variety here is the great essential. This is the difficulty that confronts all men who have to lay down a scale of diet under conditions which demand the resort to preserved foods, especially preserved meats. No matter how excellent the quality, a very short experience of the same kind of food rapidly produces an intense distaste for it. The use of milk proteids in the form of cheese, or of the vegetable proteids in the form of the leguminous plants, is, in my opinion, one of the most satisfactory aids to the solution of the problem.

As regards carbohydrates, the average allowance during the first week was 510 grams, during the second 450, with an average of 480. These were supplied in the form of bread or biscuit and potatoes as regards the starchy foods, and in the form of sugar and jam as regards the other division, sugars. As to the absolute amount of carbohydrates to be given, there is of course no regular rule to be laid down. Very much depends on the habits of the nation to which the individual belongs. In our army 1 lb. of biscuit or 1¼ to 1½ lb. of bread are traditional, and date back to the eighteenth century wars if no earlier. The Germans give 1¾ to 2⅛ lb.; the Russians as much as 2¼ lb. The form in which the carbohydrate is given is of great importance. Certain classes cannot digest a large amount of starchy food, and it was found in this experiment that the officers did not consume more than three-quarters of their biscuit ration, owing to the difficulty of mastication and conversion of the starch. The bread ration was consumed completely, but this was due to the ease with which bread can be swallowed without thorough mastication. There was a distinct craving for more bread, occasionally for more biscuit. The latter was rare, however, largely because biscuit takes much longer to eat, and is therefore not consumed all at once. As regards the officers, the demand for carbohydrates was evidenced by a craving for sugar, which lasted for a week after a return to normal diet.

As regards fats, it will be noticed that the average amount given in the first week was only 59 grams, and if we exclude the first day, when a special cheese sandwich was issued, under 50 grams. This is of course low, and the ratio of fat to carbohydrate during that period was as 1 to 10—a proportion noted in the diets of the poorer classes only. The effect of this low fat ration was distinct in the sinking of the cheeks and hollowing of the eyes of individuals. In one or two of the men and in all the officers, this was very apparent, even during the period when weights were going up. In the second part of the march the fat ration averaged 110 grams, bearing a proportion of 1 to 4½ as regards the carbohydrates. This is a high ratio under ordinary circumstances, though not higher than that which the soldier is accustomed to in barracks. The lack of fat during the first week was severely felt, as was shown by the avidity with which it was eaten during the second week. Even the high allowance of this last period was insufficient, at least as regards the officers. The craving for additional fat continued in their case for at least a week after the experiment was concluded. Foreign armies all give poor fat rations, the Russians as low as 27 grams, Germans 68 ordinarily and 100 on the special war ration, French 72 on the augmented service ration. The United States alone give over 100. The lack of fat is a serious matter when preserved meat is issued. The proportion of fat to meat in fresh meat (beef sides, medium fat, as purchased, Atwater) is about 1 to 4, but in corned beef this is reduced to 1 to 10, and indeed any figure much above this makes the meat unpleasantly greasy. In my opinion, speaking I admit a good deal from experiment *in corpore vili proprio*, a ration of 150 grams of fat would not be found too much if hard work is to be done. A good form of fat ration is cheese, which at the same time helps to do away with the monotony of the protein ration, but lard or suet would also be very suitable forms of supply, since they would assist in the cooking of the other parts of the ration. I should like to elicit from the meeting a pronouncement on the question as to which is the most suitable form of food where a large amount of energy is required, and where portability also has to be considered. Proteins I do not think can be placed higher than the allowance given in the first week of the experiment—that is, 190 grams. Portability demands the use of preserved food, and a larger supply of protein in that form means distaste for the diet, which is wasted in consequence. The choice lies between fats and sugars, or a combination of the two. The Japanese in Manchuria in cold weather relied on large issues of raw sugar, with which the men filled their pockets. The energy value attained in this manner was, I believe, somewhere in the neighbourhood of 6,000 calories or even higher—a figure demanded by the extraordinary cold to which the men were exposed. Sugar has the great advantage that it can be consumed and appreciated raw. Personally I found during the experiment that a mixture of sugar and jam in approximately equal parts formed an excellent and most sustaining diet. Fat is less portable, especially in a hot climate, and cannot of course be eaten by itself or without cooking to any great extent. The problem, as I should put it, is as follows: The energy demanded being 5,000 calories—say, for instance, a forced march of twenty-five miles under severe weather conditions—of which 800 calories is furnished by protein and 1,800 by starchy food (biscuits, as was the case in the earlier part of the experiment), in what form should the remaining 1,400 calories be furnished, by fat or sugar? or, if, as is more reasonable, a combination is used, what proportion should the one bear to the other?

REFERENCES.

[1] Schumberg, *Sanitäts-dienst und Gesunheitspflege im Deutschen Heere*, 1909, p. 297. [2] Zuntz and Loewy, *Physiologie des Menschen*, 1909, p. 701. [3] Ibid., p. 664.

DISCUSSION.

Dr. ROBERT HUTCHISON (London) congratulated Lieutenant-Colonel Melville on the accuracy of his interesting experiment. The results, he thought, were pretty much what might have been expected, for it was evident that the calorie value of the diet was below the needs of the subjects of the experiment. He agreed with Colonel Melville that the protein ration was none too high, for whatever views one might hold as to the protein requirements of the body, all careful investigations had shown that there was an instinctive consumption of large quantities when hard muscular work had to be done. In the diets of football teams in America, for instance, as much as 181 grams of protein had been found, and in the case of Scandinavian athletes as much as 217 grams daily. As regards the question of how much carbohydrate and fat should be supplied, it was largely a question of digestive capacity, and this made it very difficult to construct hard-and-fast rations. Inasmuch, however, as most people found carbohydrates easier of digestion than fats, it was probably best to supply the larger proportion of the balance of calories in the form of a concentrated carbohydrate, such as sugar.

Lieutenant-Colonel A. M. DAVIES (R.A.M.C., ret.) congratulated Colonel Melville on the solid piece of experimental work which he had carried out, which would be of permanent value from both physiological and service standpoints. He deprecated a too rigid dietary standard based solely on the calorie value of food substances; public dietaries or rations so constructed would be apt to contain too large a proportion of carbohydrates, which were cheap, and too little fat, which was more expensive. He would not advocate a fixed ration or dietary supposed to be complete in every particular, while certain simple foods, such as bread, meat, cheese, potatoes, or biscuit,

could be eaten day after day by ordinary healthy men for an almost unlimited time; his experience was that any concentrated preparation, however theoretically valuable as combining in small space and weight the necessary alimentary constituents, would inevitably pall on the palate in a short time, and become absolutely uneatable only a little later. Let the necessary foundation, or nucleus, of the ration be provided in the shape of bread, biscuit, meat, cheese, sugar (as jam), and let variety be allowed in other food substances as they might be procurable.

Dr. CATHCART (Glasgow) thought that Colonel Melville's paper was very valuable and most suggestive. He found it difficult to believe that hard work necessitated a large intake of protein, as, judging from the examination of the excreta, work seemed to have but little effect on the nitrogen output, provided the calorie intake were sufficient. The majority of experiments, however, did show that there was a slight increase in the output of nitrogen waste matter during hard work. The speaker suggested that this increase came from broken-down nitrogenous tissue, which was essential for the normal working of the muscle, and which, of course, with hard work was freely utilized; that the repair material for this essential substance was present only in very small amount in the ordinary protein of the diet; therefore, in order to obtain a sufficiency of repair material there had to be a high protein consumption. If a perfect protein source could be found, then this high protein intake would not be necessary (vide Michaud's work on feeding dogs with dog flesh). The really important point was the quality, not the quantity, of protein supplied. The cannibal, of all men, was the only one who probably enjoyed a perfect protein supply. In answer to Colonel Melville's question as to which foodstuff should predominate in the diet, he pointed out from some recent work of his own the fact that carbohydrates were not only ready sources of energy, but that they probably played some very intimate part in the metabolism of protein tissue. As fats did not appear to be able—at any rate, with the same readiness—to take part in these metabolic changes, and as carbohydrates were both cheaper and more readily digested, the speaker would supply the bulk of the extra calories demanded in the form of carbohydrate.

Dr. HALDANE (Oxford) said that the experiment described by Colonel Melville formed a most valuable contribution to the question of dietary standards. The standards of energy value in the daily food, as laid down by Atwater, were based on observations of what men actually ate when their supply of food was not limited; and it might be contended, and had been contended by Chittenden, that they usually ate more than was sufficient. Colonel Melville's experiment afforded very strong evidence to the contrary. The speaker having himself examined the men before they started, and again at the end of the march, could confirm Colonel Melville's conclusion that many of the officers and men showed evident signs of deficient nutrition. Only in the case of some of the lighter men were these signs absent. He considered the basis of Atwater's standards of energy value in food was absolutely sound, and that the records of cases in which men were placed on a restricted diet showed that not only was weight lost, but working capacity and resistance to epidemic and other diseases was greatly diminished. He would like to emphasize strongly the diminished resistance to infection. This was a factor of enormous practical importance when the food supply of large numbers of persons became for any reason restricted. With regard to standards of protein food, he did not think there was as yet any clear evidence that perhaps half as much protein as was usually consumed by Europeans would not suffice. The experimental data which Chittenden had brought forward on this point seemed to him to possess much weight; and there was no physiological improbability in assuming that a large proportion of the energy usually afforded by oxidation of protein might not be obtained without disadvantage from carbohydrates or fat. The proportion of protein in a diet, like the proportion of fat, was probably to a considerable extent a matter of chance, although a certain minimum of protein was undoubtedly necessary.

Dr. ALEXANDER HAIG (London) stated that his experience lay midway between the figures of Atwater and Chittenden. He found that his patients could not go for long periods below 90 and 95 grams of protein per day without breaking down. Some, it was true, did not break down for eighteen months, but then they became weak and anaemic. As regards the proportion of carbohydrates and fats, he referred to the proportions of these substances in the *normal* diet of man (nuts and fruits). In these the proportions found were:

	Proteins.	Carbohydrates.	Fats.
Nuts ...	... 1	0.7	3.2
Fruit ...	... 1	17.0	0.6

The one balanced what was wanting in the other, and in a mixed diet of nuts and fruit—namely 12 oz. nuts and 3 lb. fruit—a sufficient protein diet for a man of 133 lb. was found.

Proteins.	Salts.	Carbohydrates.	Fats.
1	0.5	3	2

This, then, was physiologically correct in all but the fats, which were double or treble the ordinary physiological figure. In a word, one could take too little fat in ordinary diet, and hence, perhaps, had to increase it at once when treating phthisis. Possibly all this fat was not absorbed, and it certainly did not lead to obesity or increase of the body fat. This was seen in the case of the natives of India, who took large quantities of fat and yet kept moderate or thin. Taking what physiologists might consider an excess of fat was often a cure for "the English disease"—constipation.

Dr. BUCKMASTER (London) doubted whether the primary increase of weight in the early part of the experiment could be attributed to retention of protein. In his opinion, except in the case of convalescence, it was known to be exceedingly difficult, if not impossible, to store protein, and, further, even if it were allowed that this could be retained, simple measurements of the girth of chest or calf could give no evidence of this. He further considered that the amount of fat in human dietaries was a peculiar rather than a general feature of the dietary, since the fat supply for all other animals was very small compared to the carbohydrate supply. The capacity of the individual for utilizing carbohydrate was known to be greatly influenced by simultaneous administration of alcohol. This diminishing of the power of the organism to utilize the administered sugar, evidenced by alimentary glycosuria, was well demonstrated if the carbohydrate, as in the case of the Japanese, were largely cane sugar.

Dr. JOSEPH SMYTH (Dublin) did not think that sufficient appreciation had been shown to the enormous value of Chittenden's experiments. It was not fair to put against Chittenden's experiments with different classes of men over very prolonged periods, Colonel Melville's experiments of six or twelve days. It seemed not to be borne in mind that Chittenden advocated for the success of his restricted diet that the change should not be too suddenly made, and that the diet should be as varied as possible. It was stated in the recent report of the Vagrancy Commission that Atwater had read a report showing that two workers in his laboratory got better results from a diet of 80 grams of protein daily than on the higher standard. Besides, it was not fair to put famine and starvation against a properly "restricted" diet.

Professor A. B. MACALLUM (Toronto) criticized the view that we should include the instinctive tendency towards proteins as a factor in the composition of a diet. The extraordinary diet of the eighteenth century could be thus defended, although it could not be followed to-day. The character of the composition of a diet should be based on the results of observation alone, and not on the tendency or fashion, erratic or otherwise, of individuals or classes for a period.

Lieutenant-Colonel MELVILLE, in reply, said: I must, in the first place, thank the Section for the very favourable reception they have accorded to my remarks. I should like to say that, though the paper is my own work, I have been greatly indebted to Dr. Haldane and Dr. Pembrey

for much advice and friendly discussion, as well as to Captain Walker of my own corps, for assistance in getting together the figures on which my remarks have been based. I had not the slightest intention of comparing my experiments with those of Professor Chittenden, with which some of the speakers have contrasted them; but since the theories associated with that distinguished observer's name have been rather prominently brought forward I feel that I can hardly sit down without declaring the attitude in which I regard them. Personally I am a small protein eater, and for men engaged like myself in a purely sedentary life I am inclined to agree with these theories; but when I come to the question of the diet of men leading the life of the soldier on service and exposed to the hardships that that expression implies, I confess that I can in no way accept them. As it happens there does exist at the present day a race of men who for as many centuries as we have any record have dieted themselves on a scale in every way consonant with the principles of Professor Chittenden. I refer to the inhabitants of Bengal.

A very excellent examination of the dietary of this people has been made by Captain D. McCay, of the Indian Medical Service.[1] He carried out a series of observations on Bengalis of various classes whose protein metabolism corresponded to 0.113 gram of nitrogen per kilogram of body weight per day. This meets fairly closely the demands of Mr. Chittenden, who states that "a daily metabolism of 0.10 to 0.12 gram of nitrogen per kilogram of body weight is quite adequate for physiological needs, provided a sufficient amount of non-nitrogenous foods is taken to meet the energy requirements of the body." Here, then, we have a whole race of men who for many centuries have been living on a dietetic system strictly in accordance with this rule; and here, therefore, we should have its effects clearly manifested. Captain McCay shows distinctly that the Bengali is in the first place of inferior physical development, being 25 per cent. lighter than the average European, with a smaller chest measurement, though much the same stature. The blood of the Bengali contains about 25 per cent. less haemoglobin than that of the European, and flows at 25 per cent. less pressure. As compared with Anglo-Indian and Eurasian students, the Bengali students showed no progress in development during their period of study between 16 and 20 years of age, though the several classes live in much the same surroundings, differing only, or at least chiefly, in the fact that the Anglo-Indian and Eurasian students consumed about 0.19 gram of nitrogen per kilogram of body weight per day, as against the 0.11 of the Bengali. As regards physical endurance and activity the Bengali labourer is enormously inferior to the European. Insurance offices in Calcutta rate all Bengali lives as being five years worse than European lives, and one large office will accept the policies of well-educated Bengalis of the higher castes only, and even then not beyond the age of 35 to 38 years. A very strict medical examination is made, a slight excess measurement round the waist being sufficient to ensure rejection. At the same time kidney disease is twice as prevalent amongst native patients in the Medical College Hospital, Calcutta, as amongst European patients, though scarlet fevor is unknown in India. In powers of resistance to disease I am certain that no officer with any prolonged experience of India will deny that the native is infinitely inferior to the European, living under the same conditions of climate, though these are strange to the latter. I have heard officers with experience of Central Africa make the same remark as regards the negro inhabitants of that part of the world. Livingstone, I believe, noted too that the meat-eating negro was fiercer and more warlike than the vegetable eater. Confining ourselves, however, to the Bengali, the subject of Captain McCay's observations, we have in this race the best exemplar that can be imagined of Mr. Chittenden's theories. Here, if anywhere in the world we should find a race rejoicing in that "healthfulness and longevity that are the rewards of the successful pursuance of a temperate life, modelled in conformity with Nature's laws," which we may here, I think, safely conjecture are identical with Mr. Chittenden's advice. Unfortunately the insurance companies, with an eye to their dividends, decline to recognize either the healthfulness or the longevity, in the only practical method open to them. Rather they with wilful blindness penalize the healthful and long-lived (as he should be) Bengali, as against the European, though the latter must or should be suffering from "gastro-intestinal disturbances, indigestion, intestinal toxaemia, liver troubles, bilious attacks, gout, rheumatism, to say nothing of many other ailments associated with the habitual over-eating of proteid food." Professor Buckmaster drew attention to the difficulty of attributing the rise of weight exhibited during the earlier days of the experiment to laying on of protein since it was well known that in the grown man protein is the most difficult of all tissues to increase. This, of course, I accept as an ordinary rule, but I would submit that under the peculiar circumstances of the experiment there was some reason for anticipating the possibility of such an increase. In the first place there was a considerable increase in the daily amount of protein ingested, probably about 30 per cent. As is well known a sudden increase in protein is followed by a definite retention of nitrogen for a few days before equilibrium is attained. At the same time there was a distinct stimulus to muscle growth as a result of the daily marching, and good reason to suppose that such growth did actually occur, since as already stated the measurements of the abdomen (lumbar muscles) and calf did as a matter of fact show an increase during this period. It is true that these measurements cannot be taken as a measure of the exact extent of that increase, but they are good corroborative evidence of its occurrence. Of course in any case, the increase in weight being theoretically due to an increase of muscular tissue, must be attributable, as regards at least four-fifths of its amount, to a retention of water in the body. I confess that the explanation is only tentative, and demands further investigation of the point raised. It is, however, to my mind the most plausible that can, in view of the evidence attainable, be at present brought forward. On the point of military rations being as a rule insufficient, as suggested by Professor Haldane, I should like to give a word of warning. In war the soldier is given as much as can be obtained. Often this must be insufficient for his wants. In the war lately waged by the Germans against the tribes of South-Western Africa the ration issued after the outbreak of hostilities was only about three-fourths of the value of that issued to the Protectorate troops prior to the war. Again, this war ration was only available for troops on the railway. Troops actually in the field engaged in active operations received what was called the two-thirds ration of about 3,000 calories. Thus we have the paradox that the ration of the men varied inversely as the work demanded of them. This was not because the fact that a high ration is necessary for hard work was not recognized by the German authorities, but because the very conditions that implied hard work, impeded supply. All nations lay down a certain scale of rations for issue on field service, but this must be looked on as merely a minimum scale. So much the supply department can arrange to carry and furnish. The military authorities on the spot will, in addition, procure locally by requisition or purchase, not merely what the soldier needs, but as much as they can get, irrespective of any standard of energy requirements or energy value. In conclusion I can only thank the Section very sincerely for the kind reception accorded to my paper, and express my appreciation of the very interesting discussion of which I have been privileged to furnish the subject.

[1] No. 34 of the Scientific Memoirs of the Officers of the Medical and Sanitary Departments of the Government of India, New Series, 1908.

DEMONSTRATION ON TONOMETRIC DETERMINATION OF DISSOLVED GASES.

By August Krogh, Ph.D.,
Lecturer on Physiology at the University of Copenhagen.

Dr. Krogh described the principles of tonometric determination of gases dissolved in fluids, and demonstrated two new gas tonometric apparatuses. One of these was essentially of Pflüger's pattern, but the volume of gas used had been reduced to 4 to 5 c.cm., while the surface available for diffusion was about 130 sq. cm. By means of this apparatus a tension determination on blood or any other fluid could, it was pointed out, be made in twenty to thirty minutes, during which time about 400 c.cm. of the fluid must pass through the apparatus.

The second apparatus was a so-called microtonometer. The gas volume was reduced to a bubble of 4.5 c.mm. suspended freely in the fluid to be examined. A current of the fluid, emerging from a very narrow opening, was directed against the bubble, which was thereby put in a revolving motion. The tensions could in this apparatus be completely equalized in five minutes or less, and not more than 5 to 15 c.cm. of the fluid needed to be employed. The bubble could be drawn into a very narrow graduated tube where its length could be accurately measured, and after detachment of the tonometric appliance the same apparatus was used as a microanalysis apparatus. The bubble was put in contact with suitable reagents, by which first the carbonic acid and then the oxygen are absorbed. After each absorption the bubble was drawn up into the graduated tube and measured anew, and the percentages of the two gases could then be calculated from the readings. The determinations of the O_2 percentages could be made accurate to within 0.1 per cent, and of CO_2 to within 0.2 per cent. It was, however, a very difficult matter to use the microtonometer for tension determinations on the blood of mammals, because the blood was very apt to coagulate within the narrow tube.

THE FORCES GOVERNING THE GAS EXCHANGE IN THE LUNGS.

By August Krogh, Ph.D,
Lecturer on Physiology at the University of Copenhagen.

By means of the apparatus which I have shown you I have endeavoured to solve the problem about the forces governing the gas exchange in the lungs. When I first undertook this investigation, in collaboration with my wife, we had no doubt that Bohr's theory of gas secretion was in the main correct. We expected to find that the oxygen tension in the arterial blood could, in certain conditions, be higher than in the air of the lungs, while the tension of CO_2 could be lower, and we hoped that we should be able to define those conditions more precisely than it had been possible before. In this, however, we were entirely disappointed. We did not find anything which was not in complete accordance with a simple diffusion hypothesis; and after having critically examined the experimental evidence upon which the secretion theory is based, and tested both theories by applying them to all the facts known about the gas exchange in the lungs, we were led to give up the secretion theory altogether.

We began by comparing tonometrically the tensions of O_2 and CO_2 in the arterial blood of rabbits with the percentages of the same gases in the air of the lungs. The tension of CO_2 in the blood was always found to be practically identical with that in the alveolar air. When it was altered artificially in the air the blood followed suit. The O_2 tension in the blood was in all cases lower (by 1–4 per cent.) than in the alveolar air, and also followed any alteration in the latter produced artificially.

I have made a number of experiments on tortoises. These animals are especially adapted for this kind of work in so far as it is possible to work on the air as well as the blood of one of the lungs separately. It had been found previously, in experiments published by Dr. Maar, that after division of one of the vagi the respiratory exchange rose considerably in the corresponding lung, and this was supposed to be due to some influence of the nerve upon the gas-secretory processes. I found, however, by measuring the flow of blood in the left pulmonary vein, that the increased respiratory exchange was due solely to an increased flow of blood. The tensions found were in all cases in accordance with the diffusion theory.

When a tortoise breathes air rich in carbonic acid in one of the lungs, while the other is supplied with ordinary air, the carbonic acid is not liberated but absorbed by the blood, as soon as its tension is above 2 to 3 per cent., and, when it breathes air poor in oxygen, this gas is given off from the blood, when the percentage in the lungs falls below 1.5 to 2 per cent. When the tensions are determined in the venous blood of the pulmonary artery, just those values are found. When the lung is exposed to an atmosphere with an oxygen tension lower than its own it will give off oxygen, but as soon as the tension in the air is higher it will absorb. We find diffusion, and nothing but diffusion.

Now, I do not think that the diffusion theory can be proved or the secretion theory disproved by experiments of this kind alone. Even if we find in all cases that the tensions observed are in favour of diffusion, it is quite conceivable that the processes are, nevertheless, secretory in character. It remains to be investigated whether the quantities of gas passing through the epithelial surfaces of the lungs can be accounted for by diffusion or not. The study of this problem has only just been begun. Bohr has considered the tension differences between the blood in the pulmonary capillaries and the gases in the alveoli in various conditions, and I worked out by very elaborate calculations the most probable mean tension differences. I have worked out, I think, a reliable method of measuring the *diffusion efficiency* of the lungs in man, by which term I mean the number of cubic centimetres of carbonic oxide which will pass through the lungs during one minute, when the difference in CO tension is equal to 1 mm. of mercury (0.14 per cent.).

If the diffusion theory is correct, as I think it is, the diffusion efficiency of an individual will be a reliable index of the respiratory efficiency of his lungs, and I think that valuable facts could be brought to light by measuring the efficiency in a number of individuals both in normal and pathological conditions.

The only lungs which, so far, have been examined are my own, in which I found an efficiency of about 20 during rest and a little above 30 when the lungs were dilated, as during heavy work. Calculated for O_2, that would mean that I should be able to take up during the greatest strain, when the venous blood contains only 2 volumes per cent. oxygen, about 2,600 c.cm. of gas per minute, a figure which agrees well with what has actually been taken up by men in experiments involving very heavy muscular work.

The diffusion-constant for CO_2 is extremely high on account of the great solubility of the gas (in my case about 500 during rest and above 700 during work), and it is therefore a necessary consequence from the diffusion theory that the CO_2 tension in the arterial blood should always be exactly equal to that in the alveolar air. I think that it follows from the work of Haldane, Priestley, Poulton, Douglas, and others in this country, of Yandell Henderson in the United States, and from the actual determinations by my wife and myself, that the equality postulated does, in fact, exist.

DISCUSSION.

Dr. Haldane (Oxford), after referring to the great interest among British physiologists in Dr. Krogh's methods and investigations, said that the recent investigations of Dr. Douglas and himself on mice, rabbits, and men confirmed Dr. Krogh's conclusions in so far as they referred to normal resting conditions. He thought there would now be general agreement among physiologists that during rest, under normal conditions, the gaseous exchange between blood and alveolar air was by diffusion alone, and the gaseous tensions in the arterial blood was practically identical with those of the alveolar air. On the other hand, Douglas and he concluded that the physiological stimulus of want of oxygen led to actual absorption by the lungs.

Professor Starling (London) thought that most of the facts pointed to the diffusion theory as being the correct one. He admitted that the experiments of Bohr and Haldane were evidence in another direction. As for the method—the CO method—which Haldane employed, he thought that the animal could not be regarded as being in a normal condition, and that the conclusions drawn from such experiments might therefore be regarded with some suspicion.

Dr. Buckmaster (London) thought that the figures published by Drs. Haldane and Lorrain Smith on the oxygen tension of arterial blood as ascertained by the CO method were to be received with caution, since these figures were no less than nine times greater than those given in the textbooks, on the authority of Pflüger and his pupils. At high levels, Dr. Haldane considered that an active secretion of oxygen into the blood might well be an explanation of the ease with which man and animals became accustomed to a lowered tension of oxygen in the atmo-

spheric air; but, in the opinion of the speaker, might not the increase in actual haemoglobin in the volume of circulating blood suffice for this, for in the observations made fifteen years ago on Mont Blanc, in conjunction with Dr. Slater and Mr. Dent, he showed that the haemoglobin control of the blood reached 120 and more (Oliver haemoglobinometer).

Professor Thompson (Dublin) said that, in his opinion, this discussion brought us nearer to agreement on the main points than we had been any time for fifteen years past. He thought the only fundamental difference now outstanding was the possibility of active absorption of oxygen taking place under abnormal conditions.

Dr. Krogh in reply commented on the fact that the epithelial cells covering the blood vessels are very badly adapted for secretory work, as they consist of extremely thin, structureless membranes without any nuclei. He had found by special experiments that his own tonometric determinations could not be affected by any consumption of oxygen in the blood itself, but would not vouch for the determination made by Fredericq in this respect. He pointed out as a possible source of error in the recent experiments of Haldane and Douglas, that the percentage-saturation of the blood with CO when exposed to a definite gas mixture might be variable not only, as has been shown, from individual to individual, but also, perhaps, from time to time in the same individual. He wished to test the results by the direct tonometric method, and in several other conditions which might likewise put a strain on the pulmonary functions.

THE EXCRETION OF CREATINE IN DIABETES MELLITUS.

By M. Ross Taylor, M.D.,
Physiological Laboratory, Glasgow University.

[Preliminary Note.]

The course of protein metabolism in diabetes mellitus has been frequently the subject of investigation, but so far no very definite results have been obtained. This has been due in part to the fact that the diet of the patient has not been kept constant, and in part to the fact that most diabetic diets are very rich in protein, thus causing a large output of nitrogen in the urine, and in this way obscuring any small variations which might have been present as the result of alterations in the metabolic processes.

In the present series of cases—16 in number—it was found that the output of total nitrogen in diabetes was not always greatly increased even with a comparatively high intake of protein; indeed, the nitrogen output was frequently abnormally low. As Cathcart had previously shown that there seemed to be some connexion between the carbohydrate metabolism and the creatine-creatinine metabolism, it was decided to investigate the urine of diabetes for variations in the output of these substances.

As regards creatinine, it was found that the output was not increased to any considerable extent even when the patient was on a highly nitrogenous diet. Creatine, on the other hand, a substance never found in the normal urine if the diet be free from creatine and creatinine, was always found. It was present even when the patient was kept on a creatine-creatinine-free diet.

Some further work has been carried out in conjunction with Dr. E. P. Cathcart on the creatine excretion after the induction of glycosuria by means of injections of phloridzin. Here it was found that if the animal were allowed only a small quantity of carbohydrate in the diet creatine appeared in the urine during the time sugar was being excreted. It disappeared as soon as the sugar excretion ceased. On the other hand, if the animal were given very large amounts of carbohydrate, so that even during the period of glycosuria there was still an abundance of sugar in the body, then no excretion of creatine accompanied the output of sugar in the urine. It was further found that fats, even in very large amount, could not replace the carbohydrate. A liberal supply of fat combined with an amount of carbohydrate, sufficient to prevent the appearance of creatine in the urine under normal conditions, did not check the excretion of creatine when glycosuria was induced.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

URINE AND FORMALIN.

In the Journal of October 15th, p. 1164, appears a memorandum from Dr. P. Hamilton Robertson, of Glasgow, calling attention to the presence of formalin in the urine as a cause of reduction of Fehling's solution, and so forming a possible fallacy in testing for sugar.

May I be permitted to point out that this fact is well known to every one acquainted with the rudiments of organic chemistry? Formalin is an aldehyde, and, consequently, possesses active reducing properties. Glucose is, likwise, an aldehyde, and, naturally, the copper test is similarly affected by the two substances.

There is another reason for avoiding the addition of formalin to urine intended for chemical or microscopical examination. It is that formalin enters into combination with urea and forms a precipitate which adheres in a surprisingly close manner to the sides of the test tube or vessel containing the urine. Under the microscope the deposit appears as lustrous spherical bodies, not unlike leucin. In the *Practitioner* for 1903 I published a note drawing attention to this much less known phenomenon, and gave a figure of the deposit.

For several years past, in my clinical teaching, I have frequently warned students against these two misleading effects of adding formalin to urine, and have demonstrated the results in the wards of Sir P. Dun's Hospital.

Walter G. Smith, M.D.Dub.,
Physician to Sir P. Dun's Hospital; Ex-President, R.C.P.I.

CAUSE OF APPENDICITIS.

As this question is still unsettled, I venture to put forward the view that it is due to eating animal food in which the early stages of decomposition have taken place, this food being particularly dangerous when insufficiently cooked. The following may be classed as dangerous:

1. Meat kept till it is tender.
2. Game "hung" till it is "high."
3. Tinned meat and fish.
4. Nearly all the animal food on which boric acid has been used.
5. Much of the food "kept in cold storage."

A great deal of this is decomposed, or dangerously near being so, before it reaches the cook.

In times preceding the modern increase of appendicitis the first two furnished most of the decomposed animal food eaten, and the affection was comparatively rare.

Tinned meat and fish were introduced some time in the Fifties, and came into fairly general use in the Sixties or Seventies. Some years later appendicitis was noticed to be on the increase; it is quite possible that it was increasing for years before it was noticed to be so, for the advance was very slow at first. Then later boric acid and cold storage were employed to preserve meat and other food, and the increase in the number of cases of appendicitis became more marked. As the consumption of these foods increases so does the frequency of appendicitis, till now it has assumed the proportions of a veritable plague, from which no one is safe.

The actual cause of the inflammation of the appendix is probably microbial. When decomposition of animal bodies sets in a great number of varieties of bacteria appear in the tissues—the bacteria of decomposition. It is almost certain—to judge them from their association with putridity—that some of these are capable of producing disease, and if they gain admission to the alimentary canal while still active—that is, if they survive the cooking process—it is easy to understand how they can get into the cavity of the appendix and set up mischief. Irritation and inflammation follow just as cystitis follows the use of a foul catheter.

It is probable that efficient cooking would always render this dangerous food safe, but then cooking is not always efficient; and the more dangerous food there is in daily

use the more likely we are to have accidents, just as in a magazine the more loose powder lying about the more frequent are explosions.

In many instances the protection afforded by cooking is altogether absent, as in the case of articles which are eaten as they come from the tin. In every case probably the food is cooked before it is tinned, but it may become decomposed and capable of causing disease before it is consumed.

Whatever may be said of this view of the case, the broad facts remain. A great change in the food of the people in recent times. A great increase in the incidence of appendicitis—the one following the other within a short time. The recently introduced food capable of producing appendicitis, and appendicitis following its introduction.

Hove. W. COATES, M.D., R.U.I.

AN UNUSUAL CAUSE OF HAEMATURIA.

CASES of blood in the urine are, perhaps, to most practitioners of frequent occurrence, and very often present difficulty in diagnosis, for it is rare to get classical symptoms of a definite lesion. The following case is somewhat uncommon, and that is my reason for reporting it.

I was called to see a youth, aged 19, suffering from severe collapse, the result of some form of haemorrhage; when I saw him he was quite blanched. I was shown the urine he had passed; there must have been 2 or 3 pints of almost pure blood. He also complained of great pain in his back, and it looked like a case of acute nephritis with haemorrhage, although there was no increased pulse tension, no oedema, and only a slight rise of temperature. The blood continued in the urine for some days in small quantities. Under the microscope what looked like blood casts were seen, but were not distinctive. I made a provisional diagnosis of acute nephritis, no other cause being then traceable. On the fourth day the abdomen and also the penis seemed swollen. On manipulation I was able to express from the urethra a little discharge which, under the microscope, contained gonococci. On further questioning him it was found that infection had occurred about a fortnight previously.

At this time it was impossible to draw back the prepuce owing to inflammatory swelling. After a week's treatment it was possible to draw the foreskin back completely, and then the source of the haemorrhage was seen in the shape of a deep triangular ulcer on the dorsal surface immediately behind the glans, and penetrating deeply into the corpora cavernosa almost to the urethra. This healed completely, with very little scarring and no deformity. Treatment consisted in black wash locally and mercury internally, in view of a possible syphilitic infection.

Birmingham. J. F. BRIDGE, M.B., D.P.H.

INTRAUTERINE CONTUSION.

SOME two years ago you published a paper I read before the Chelsea Division on intrauterine wounds of fetal head with 2 cases. The case I now report differs from those, but is also most important from its bearings on medical jurisprudence.

Mrs. F. was delivered on September 4th, 1910, of a very small female child, weighing under 3 lb., which lived nine hours. She did not expect for another month. It was born in its membranes. On examination, the left pectoral region and the left arm were contused and blackened. The discoloration extended on front and inside of arm to an inch below the elbow. The mother stated that about three weeks previously she fell across the arm of an armchair and bruised her abdomen. There were no visible bruises on her abdomen.

Supposing the case had been attended without a doctor, he might, on being called some hours afterwards, have suspected a fall or other foul play, and thus a charge might be brought against an innocent mother or nurse.

Another rather rare complication was that a few hours after the birth a fetus of about the size of a two-month pregnancy came away with its membranes and placenta complete.

Chelsea. JAMES HAMILTON, M.D.

Reports
ON
MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF THE BRITISH EMPIRE.

KASR-EL-AINY HOSPITAL, CAIRO.

TWO CASES OF GANGRENE FOLLOWING SCORPION STINGS.

(By GEORGE THOM, M.R.C.S.Eng., L.R.C.P.Lond., late Resident Surgical Officer.)

OF numerous cases I have seen in Egypt stung by scorpions, the following two cases, in which gangrene occurred, are of interest.

The first case, of whom I publish a photograph, is also an excellent example of the condition to which the native will, in his disease, allow himself to reach before seeking advice, through fatalism, ignorance, and fear of hospital treatment:

CASE I.

An Arab peasant, aged about 26, admitted to the Kasr-el-Ainy Hospital, gave the following history of the condition of his leg, seen in the photograph. About two months previously, when going to work—walking barefooted over the desert—he felt something prick the sole of his foot. He found he had trodden upon the dead body of a scorpion, which was sun-dried and brittle. Examining his foot, he found one of the "bones" had pierced, and was protruding from the thick skin of the sole. This he removed and went to work. Later in the day his foot felt numb, and during the evening noticed it was swollen. The next morning the swelling and numbness had spread up the leg. There was no pain or constitutional symptoms, and he continued working the following two days. He then found the toes becoming black, the discoloration spreading over the area previously swollen. Subsequently the toes separated, and then the foot. The gangrenous tissue he pulled off as it loosened, and dressed the leg with tar and rags. The separation continued to the upper third of the leg, leaving the bones bare. During this time, he states, he was neither ill nor feverish, nor did his general condition on admission suggest any severe illness. After the foot separated, he found he could walk about on the bony stump, the wound at the line of demarcation healing rapidly. After some days' walking, the inferior tibio-fibular ligament parted. So as to keep the bones together, he fixed them together with string and rags. Later he found that, owing to cicatrization, his knee became bent to such an extent as to prevent him walking. This inability and the suggestion of the police made him seek medical advice at the hospital, though he lived within three miles of Cairo.

On admission, the protruding bone—fastened together at the lower end with string—were hard, polished, and blackened. The wound above had healed except for some protuberant granulations around the remains of the interosseous membrane and the margins of the bones. The knee was flexed to the angle indicated in the photograph. The patient's general condition was excellent.

Under stovaine anaesthesia flaps were formed, the necrose bones removed, and patient discharged to wear a kneeling knee-pin.

The interesting feature of this case lie nthe sudden onset of a painless spreading gangrene without constitutional disturbance.

Weir Mitchell, in his description of snake poisoning, mentions the occasional localized lesions produced, such as extravasation of blood, gangrene, and the extreme rapidity of putrefaction following such lesions after snake bites.

Gangrene following the introduction of scorpion venom is very rare. It is also mentioned in *Allbutt's System of Medicine* that small doses of snake venom introduced intravenously either produce temporary extensive coagulation of the blood or increased coagulability followed by increased fluidity. This condition, however, does not follow subcutaneous injections in which the remote manifestations upon the nervous system, and thus to the circulatory system, are the common results.

This patient, I presume, received the venom into a punctured venule of his foot through one of the cracks in the thick skin of the sole, so common in native peasants, with the result mentioned. I am indebted to Professor Madden, under whose direction the treatment was carried out, for his kind permission to publish this case.

Case II.

A native child, aged 4, was brought to the hospital stung on the foot about ten hours previously by a scorpion. The parents had incised the point of entry, and fastened a tourniquet above the knee. This tourniquet (composed of a loop of webbing) was twisted with a piece of stick so tightly above the knee that it was practically buried in the thigh. The limb below was swollen and blue—the toes black—the child was screaming with pain. Considering the condition of the limb due to the tourniquet alone I promptly cut it, and the child died instantaneously in the middle of screaming. The treatment should, of course, have been amputation. Unfortunately, there was no *post-mortem* examination to discover the condition of the blood, or whether death was due to embolism.

Scorpion venom produces severe collapse, death being infrequent except in children, and then usually some time after inoculation.

If death in this case was caused by a thrombus, that thrombotic condition may have been caused either by the tourniquet or the venom producing coagulation. The poison alone would not have caused such instantaneous death as the severing of the tourniquet produced in this case.

Reports of Societies.

MEDICAL SOCIETY OF LONDON.

Monday, October 24th, 1910.

Charters J. Symonds, M.S., F.R.C.S., President, in the Chair.

Metastatic Cancer of the Ovary.

Mr. J. Bland-Sutton, in a paper on the clinical aspect of secondary (metastatic) cancer of the ovary, said the subject of cancer of the ovary had interested him during a quarter of a century; during the whole of that period he had entertained the gravest doubt in regard to the existence of primary cancer of the ovary. Observations published during the last eight years had established the fact that solid tumours of the ovary large enough to be perceptible to eye and hand in the hypogastrium, which on microscopic examination exhibited the structure of carcinoma, were nearly always secondary to primary foci of cancer in the breast, gall bladder, or some part of the gastro-intestinal tract, especially the pylorus and colon. When patients were submitted to operation for solid ovarian tumours, especially if the tumours were bilateral and accompanied by vomiting and ascites, a careful examination should be made of the abdominal viscera, especially the stomach and sigmoid flexure of the colon. When cancer attacked the stomach, caecum, or gall bladder, the cancerous cells permeated the muscular and serous coats in relation with the primary growth, and found their way into the general peritoneal cavity. It must not be imagined that the implantation of cancer on the walls of ovarian cysts was an uncommon event. Within a period of eighteen months (from March, 1909, to October, 1910) seven examples had come under his observation. One of the most striking features in the after-history of patients from whom malignant ovaries had been removed was the rapidity with which the cancer recurred in the pelvis, even in cases where the uterus and pelvic peritoneum seemed free from infection at the time of the operation. When the primary disease was too advanced for removal, the patient's condition was often made more bearable, if not comfortable, by the removal of the ovarian masses, but it must be borne in mind that in attempting to do that the lives of patients were often shortened. A prolonged study of the question had strengthened his conviction that primary carcinoma of the ovary remained unproven. Dr. Bernard H. Spilsbury, who read a paper on the pathology of certain tumours of the ovary, said he had been unable to obtain confirmation of the view held by Mr. Bland-Sutton. Dr. McCann said that in his experience patients operated on for ovarian carcinoma did not live longer than two years afterwards. Mr. Malcolm said that all the evidence he had pointed to the correctness of Mr. Bland-Sutton's views.

OPHTHALMOLOGICAL SOCIETY OF THE UNITED KINGDOM.

Thursday, October 20th, 1910.

Dr. G. A. Berry, President, in the Chair.

Marginal Keratectasia.

Mr. George Coats described the pathological features of a case of marginal keratectasia formerly shown to the Society by Mr. J. H. Fisher. They consisted essentially in a loss of the superficial lamellae in the periphery of the cornea, and their replacement by a loose vascular fibrous tissue. The membrane of Descemet had undergone enormous compensatory thickening, and showed many ruptures plastered over with new layers of hyaline substance. In one area the epithelium was oedematous and desquamating, either as a result of commencing ulceration, or of leakage from the anterior chamber. From a study of recorded cases it was shown that marginal keratectasia is usually the result of "furrow keratitis" (Schmidt Rimpler), of "marginal atrophy of the cornea" (Fuchs), a condition in which, without ulceration, a groove develops in the upper periphery of the cornea, usually between an arcus and the limbus; ectasia, distortion of the cornea, and astigmatism against the rule, are produced by the giving way of the floor of the groove. In respect of pathogenesis, the speaker mentioned two opinions: (1) That the condition was purely degenerative; and (2) that it was inflammatory. In favour of the first there was the common absence of injection clinically, and of round-cell infiltration microscopically, as well as the association with arcus senilis; in favour of the second, the occasional presence of injection and even photophobia, or of pseudopterygium or pterygium; and the fact that vessels crossed the floor of the groove. Actual ulceration was probably excluded by many cases carefully watched over long periods. It was possible that a combination of these two explanations was correct—namely, that the products of degeneration set up a low form of inflammation, or that a low form of inflammation induced degeneration.

The Membrane of Bruch.

Mr. George Coats also read a paper on an unusual instance of colloid excrescences of the membrane of Bruch. The case was reported chiefly because it afforded an opportunity, rarely obtained, of comparing an ophthalmoscopic appearance with its histological foundation.

The patient, aged 58, was achondroplasic and had glaucoma in both eyes. V. R.: Fingers at 3 ft. H. + 7 D. V. L.: Perception of light. In the right eye the media were fairly clear, and the fundus was strewn in every part with flat, scaly, irregular, white or grey areas, with a slightly wavy surface, and with distinct glitter on moving the light. The borders were sinuous and rimmed with pigment; pigmentation also occurred on the surface, especially towards the periphery. The choroid generally was degenerate. In the left eye there was a detachment of the retina; this eye was subsequently excised. Pathological examination showed that the areas were due to an aberrant type of colloid excrescences of the membrane of Bruch. In addition to the ordinary rounded forms, flakes and irregular spongeworks of colloid material were found. The choroid was very degenerate, and showed evidence of the former presence of a flat detachment. All the measurements of the globe were notably small, and in other respects a degree of infantilism was suggested.

In discussing the relationship of these conditions with the glaucoma, the speaker pointed out that the glaucoma itself was peculiar in that there was no adhesion of the corneo-iridic angle, and, so far as could be seen, no hindrance to

drainage. The rise of tension might be due simply to a disproportion between the secretion and the amount of drainage available—normal so far as it went, but insufficient. The other eye, however, though glaucomatous, was not unduly small, at least in its external measurements. Another possibility was that the iris root had been in apposition with the ligamentum pectinatum, but not adherent, and had come away in the fixing fluids. Or there may have been an alteration in the chemical nature of the fluid to be drained.

Reviews.

OPHTHALMIC SURGERY.

THE Reports of the Eye Clinic of the Caroline Medico-Chirurgical Institute of Stockholm open with an obituary notice of the late Professor WIDMARK, the editor and founder of the Reports of the Eye Clinic of the Caroline Medico-Surgical Institute,[1] and Professor of Ophthalmology at the Institute, who passed away on December 15th, 1909. Widmark's name will always be associated with his researches upon the influence of ultra-violet rays upon the eye, upon the nature of jequirity ophthalmia and of gonorrhoeal ophthalmia, and upon the pathogenesis of dacryocystitis and ulcus serpens. He was greatly interested in school myopia, and introduced the sodium salicylate treatment of sympathetic ophthalmia. As a teacher he was unrivalled in Sweden, and the whole nation is the loser by his early death at the age of 59.

The first paper describes a new keratoscope invented by Dr. Hans Gertz of Helsingborg. The instrument is an ingenious adaptation of the well known Placido's disc, which is so useful in demonstrating an irregular astigmatism. The human eye is, however, not sensitive enough to detect the difference in the shape of these rings caused by a low degree of astigmatism. Gertz has been able to make the distortion more evident—first, by using an oval disc instead of a round one, and secondly, by allowing the oval ring to rotate. The disc is fitted to the end of a tube furnished with a magnifying lens, and can be easily rotated with the finger. The angle of rotation can be read upon a graduated arc. The eye is observed with the instrument and the point determined where the image is most circular, and again where it is most oval. This gives the angle of the corneal astigmatism. Then a guess is made of the amount of distortion, and from this the astigmatism is estimated. Four discs are used, each one slightly more elliptical than the last. For slight degrees of astigmatism the slightly elliptical disc is employed. For higher degrees the more elliptical discs are suitable. With a little experience the amount of astigmatism can be accurately gauged.

Dr. Lamm records a case in which orbital phlegmon and total optic nerve atrophy followed syringing out the lacrymal sac. Such accidents are not infrequently seen in countries where trachoma is endemic, and the sac infected and friable, but they are rare in Europe.

Widmark contributes a paper which he heads "Minor Ophthalmic Observations." He records a rare form of conjunctivitis, a benign tumour of the iris, and then discusses the etiology of spring catarrh. He observes that if the disease be due to the action of ultra-violet rays, it ought to be very common in Northern Sweden and Norway during the long summer. He wrote to several ophthalmologists practising in the northern towns, but they all agreed that the disease was most rare in their clinics. Widmark thinks, therefore, that ultra-violet light is not the cause of spring catarrh.

Dalén writes upon evulsion of the optic nerve, which is defined as a forcible dislocation of the nerve from its scleral canal. It is a rare injury. The author adds a case of his own. The eye was enucleated and histologically examined. Two photomicrographs illustrate the paper.

The late Professor Widmark's paper on "Delayed Reformation of the Anterior Chamber after Cataract Extraction" is of great practical importance. The subject has received very little attention, and yet in about 2 per cent. of all operations late closure of the wound will be noticed. It is usual when the first dressing is made to find that the anterior chamber has formed again, but even after operations which are technically correct the wound edges may not have united at the end of the first week, or later. Widmark gives a tabulated list of examples of late union which is compiled from the records of the Seraphim Lazarette at Stockholm. In one case the anterior chamber only reformed on the sixtieth day.

Cocaine has by some been held to be the cause of this abnormal delay in healing, but Widmark thinks the complication is far more frequently observed after corneal than after scleral incisions, and he ascribes it to deficient technique. Very frequently a tag of capsule prevents correct apposition of the wound edges. Delayed union may end in various ways. Most frequently there is no sign of any inflammation, and sooner or later the wound closes, and the final result is uninfluenced. An opacity of the cornea may develop. This may be slight or may be sufficiently dense and widespread to necessitate an optical iridectomy. Or the eye may become infected, and panophthalmitis supervene. The wound may never heal at all, a permanent fistula persisting, followed by gradual decline in visual acuity, until there is complete blindness. Finally, glaucoma may appear. This often comes on after the patient has been lost sight of, and is more frequent after combined than simple extraction. The best treatment is to keep the eye bandaged and await events.

Lindahl contributes a lengthy paper upon traumatic cysts of the conjunctiva, which is illustrated by several woodcuts in the text and by a coloured plate.

Professor Dalén's second paper is a clinical contribution referring to the development of cataract after electric shocks. Cataract following lightning stroke is not included. All cases recorded so far have received a discharge of high potential, in one case 21,000 volts, in the head.

The volume ends with a study of the part played by the anterior part of the vitreous in the embryology of the cornea by V. Knape of Helsingfors.

[1] *Mitteilungen aus der Augenklinik des Carolinischen Medico-Chirurgischen Instituts zu Stockholm.* Von Dr. J. Widmark. Elftes Heft. Jena: Gustav Fischer. 1910. (Sup. roy. 8vo, pp. 142, Taf. 2, Abb. 15. M. 7.)

MICROSCOPICAL EXAMINATION OF THE EYE.

THE third edition of Professor GREEFF'S guide to the microscopical examination of the eye[2] will be warmly welcomed by all ophthalmic students. The first edition was translated into English and Japanese, and has been recognized as by far the best book extant on the technique of ocular pathology. In the third edition Professor Greeff has obtained the assistance of Professor Stock of Freiburg, and of Professor Wintersteiner of Vienna, and in consequence the book may be held to present the best methods in use in Berlin, in Vienna, and in Freiburg. The pages dealing with the examination of secretions and bacteriology are written by Professor Stock, whose association with the Freiburg School of Ocular Pathology is a sufficient guarantee of the excellence of the directions he gives. Professor Greeff's work contains all that is necessary to enable a beginner to learn the details of the microscopy of the eye. The instructions are clear and precise without being verbose. Only sound practical methods which the writers have proved by experience are given, and the beginner is advised to master one or two of them and become proficient in them rather than to waste time over many methods. The book is divided into three chapters, dealing with general details, special methods, and bacteriology, in the order named. Professor Greeff tells us that the necessary armamentarium is small, an accurate technique is more essential than expensive and complicated instruments. It is easier to get satisfactory sections from a well-imbedded specimen with a cheap simple microtome than from carelessly imbedded tissues with an expensive instrument. The author thinks that in such a small subject as ophthalmology every expert should be able to prepare his own sections and decide upon their nature. He lays stress upon the necessity for ex-

[2] *Anleitung zur mikroskopischen Untersuchung des Auges.* Von Professor Dr. R. Greeff. Dritte Auflage. Berlin: August Hirschwald. 1910. (Demy 8vo, pp. 158. M. 4.)

amining tissues in their fresh unstained state, an examination which is carefully taught to all German students, but has been much neglected in England. Professor Greeff's book should be in the hand of every student in an ophthalmic pathological laboratory. It is by far the best book which has come under our notice—short, concise, and yet exhaustive. It is written in easy German which should be within the compass of any man who intends to take up a speciality.

TRACHOMA INFECTION.

PROFESSOR HERZOG'S monograph upon the etiology of trachoma[3] is well worthy of careful study, and opens out a wide field for research. As is well known, v. Prowazek, the late Professor Schaudinn's assistant, travelled to Java, and, working with Halberstädter, discovered in cells obtained from cases of trachoma certain inclusions, small ovoidal granules, which he held to be protozoa, and which he called "chlamydozoa." The bodies he regarded as the organism which gave rise to trachoma. These inclusions have been studied by Greeff and others, and there is now no doubt that they are often present in cases of recent untreated trachoma. Heymann then detected the same cell inclusions in the conjunctival cells of eyes suffering from gonorrhoeal ophthalmia and ophthalmia neonatorum. This discovery has been confirmed by Flemming and others, and it may be consided to be proved that the so-called trachoma bodies are also found in non-trachomatous gonococcal blenorrhoea. V. Prowazek and Halberstädter, Schmeichler, and Lindner, accepting Heymann's work, would have us believe that there is an "acute trachoma of infants," which is non-gonococcal, caused by the chlamydozoon. But Herzog points out that they have overlooked the fact that Heymann found these bodies in cases in which the presence of the gonococcus had been clearly proved by history and by cultural methods. Herzog believes these chlamydozoa not to be protozoa at all, but metamorphs of the gonococcus. He shows how old cultures of the gonococcus which have been passed from tube to tube develop involution forms. First a small metamorphic form develops, and then small bodies exactly like the cell inclusions of v. Prowazek. Among them, too, forms may be found like bent dumb-bells (*Hantelform*). All these involution forms can be detected in cases of blenorrhoea gonorrhoeica. In an example cited at first only normal gonococci were present, then the first involution form was found, and finally all macrogonococci disappeared, and only dumb-bells and microgonococci, the cell inclusions, remained. Finally these two disappeared as the case got well.

Herzog made the following clinical experiment. A pure culture of urethral gonococcus was prepared, and a loopful placed in the conjunctival sac of an eye which had been blinded by absolute glaucoma (the condition of the second eye is not stated). The eye was free from trachoma. The result was severe gonococcal ophthalmia. Fourteen days later typical trachoma bodies appeared. He admits that gonococci are not found in acute trachoma, but he believes that the "microgonococcus," the cell inclusions, are able to obtain a footing in the conjunctiva, and cause trachoma.

Herzog enters fully into all the objections which might be made to his theory, and his conclusions are of a most suggestive and interesting character. There is, however, much against the hypothesis. In the first place, gonococci are practically never found even in the most acute trachoma which is seen in Palestine and the East. Again, in these countries gonococcal ophthalmia obtains as a most violent summer epidemic, so severe that a very large proportion of the cases have corneal ulcers, and at least half the gonococcal ulcers perforate. Yet these severe cases, which often drag on for months, do not lead to trachoma. It has been shown that this gonococcal ophthalmia is not secondary to urethral infection. It is a pure eye disease, and is rampant in districts where gonorrhoea is quite unknown, and where ophthalmia neonatorum is never seen. It is, however, evidence in favour of Herzog's views that in Palestine and Egypt, where fully 90 per cent. of the population have trachoma, this gonococcal blenorrhoea is so prevalent.

Before the theory can be accepted as proved it will be necessary to produce trachoma by inoculating normal eyes with the microgonococci which Herzog has observed in old cultures, or from an old case of gonococcal ophthalmia which has no gonococci left, but only microgonococci.

These investigations would be best carried out in Palestine or Egypt, where trachoma is far more acute than in any European land, and where the material is endless in amount.

[3] *Ueber de Natur und die Herkunft des Trachomerregers und die bei seiner entstehung zu beobachtende Erscheinung der Mutierung des Gonokokkus Neisser.* Von Prof. Dr. Hans Herzog, Berlin. Berlin und Wien: Urban und Schwarzenberg. 1910. (Sup. roy. 8vo, pp. 56. M.5.)

THE OPHTHALMOLOGICAL SOCIETY.

THE second fasciculus of the thirtieth volume of the *Transactions of the Ophthalmological Society of the United Kingdom*[4] consists almost entirely of records of interesting and rare cases. An exception is Mr. M. L. Hepburn's paper on night blindness. J. H. Tomlinson describes his scotomagraph with stereoscopic fixation. The volume is well illustrated, and contains two very beautiful coloured plates. One illustrates Mr. Hudson's case of an unusual form of retinal disease; the second Mr. Lawford's case of episcleral tubercle and Mr. F. Moore's case of subretinal haemorrhage. Mr. Hepburn in his paper divided xerosis into two groups, one associated with general disease, the other a sequel to a local affection. The first group contains a mild form associated with night blindness and a severe form known as keratomalacia. The paper deals with the first form, idiopathic night blindness. The cases present the following characteristics: Hemeralopia, patches of xerosis, and occurrence of these symptoms between May and September. The complaint is always found in children, and generally in boys. The cases are usually from poor districts. The cornea is always bright and clear. The children are not conspicuously ill-nourished, but are often remarkably healthy-looking. Mr. Levy also finds pallor of the fundus, but Hepburn has not been able to substantiate this. The fields are full, there is no ring scotoma. Hepburn rejects malnutrition as a cause. Perhaps a weak metabolism of the visual purple accounts for the hemeralopia. In favour of this view is the fact that if the eyes be tied up all day the night vision is improved. Albinos do not suffer from night blindness, so we must conclude that the pigment epithelium can regenerate its visual purple without its pigment. The paper is a useful summary of the subject. Major Mould stated that he had seen many cases of night blindness in India. All had the intense retinal pallor described by Mr. Levy. They all improved on strychnine and quinine. Mr. Stephenson had found slight general contraction of the visual fields and a contraction for red and green. Mr. James describes a case of panophthalmitis in which, among other organisms, the *Bacillus aërogenes capsulatus* was found. This produced gas in the eye, which escaped in bubbles from a spot on the limbus.

MENTALLY DEFICIENT CHILDREN.

TEN years have elapsed since the second edition of Dr. SHUTTLEWORTH'S book on *Mentally Deficient Children*[5] was published. A new edition has been called for, and Dr. Shuttleworth has secured the co-operation of Dr. Potts, who was a medical investigator to the Royal Commission on the Care and Control of the Feeble-minded, in revising this third edition. There is no doubt that this collaboration has proved successful, for the book contains the knowledge of what has been done in former years for the improvement of the mentally deficient class as well as an up-to-date acquaintance with more modern movements.

The report of the Royal Commissioners is frequently referred to, and a chapter has been added on the medical examination of children requiring instruction. The Royal Commissioners recommend that the Lunacy Commissioners

[4] *Transactions of the Ophthalmological Society of the United Kingdom.* Vol. xxx, Fasc. II. London: J. and A. Churchill. 1910 (4s.)

[5] *Mentally Deficient Children.* By G. E. Shuttleworth, B.A., M.D., and W. A. Potts, B.A., M.D. Third edition. London: H. K. Lewis. 1910. (Cr. 8vo, pp. 254, 18 plates and other illustrations. 5s.)

should be replaced by a larger body called "The Board of Control"; they also give various recommendations and definitions with regard to the classes of persons needing care and control. The recent microscopical investigations of the brain of cases of mental defect made by Drs. Bolton and Tredgold are alluded to, and we learn that cellular changes have been found in all regions of the brain, and sclerosis in localized patches.

The prominent pathological conditions of the several types of congenital mental deficiency are now divided into ten classes, instead of seven, the number in the second edition. The two interesting groups of cases spoken of as "word-blind" and "word-deaf" are described, and so are the conditions of "infantilism," which Dr. John Thompson so well pointed out. There is also a very good table for distinguishing the differences between Mongolism and cretinism, which is of much practical importance.

The chapter on the medical examination of children requiring special instruction, as we before mentioned, is a new one. Full information is given with regard to the education of mentally defective children in England and Wales, in Scotland, and in Ireland. The method of examination of children deemed to be defective is described, and the form of certificate prescribed by the Board of Education with regard to defective children is given at length. The *modus operandi* for the medical inspection of mentally defective children is fully described.

It has been found necessary to form after-care committees to look after the children when they leave school at the age of 16, and the results have been very encouraging, but opinion is rapidly growing that the permanent care of the feeble-minded is absolutely necessary.

There are appendices which give useful information respecting the public and charitable institutions in Great Britain and Ireland, in America, the Colonies, and many Continental countries, for mentally deficient children and adults, and there is a copious bibliography. The illustrations are very good.

We have nothing but praise for this book. It will be of use to the medical man, the teacher, and to the increasing number of philanthropists and others who are interested in the care, treatment, and training of mentally deficient children.

PULMONARY TUBERCULOSIS.

DR. BARDSWELL'S *Advice to Consumptives on Home Treatment, After-Care, and Prevention*[6] contains much sound and practical advice from a man of wide experience in the matters of which he treats. It is chiefly intended to guide the consumptive who has had the advantage of treatment in a sanatorium and is desirous of knowing how to proceed after he has left the control of the institution. The substance of the book consists of a series of lectures given to the patients at the King Edward VII Sanatorium, of which Dr. Bardswell is the Medical Superintendent. The first chapter sums up in brief the main points as to the nature of consumption and the *rationale* of its treatment. Then follows advice as to fresh air; food; rest, exercise, and recreation; occupation; with a chapter on the various factors in sanatorium treatment and one on infection and disinfection. There are some very sound remarks about recreation and games, about clothing, and about temperature taking, among other things. A chapter on emigration contains much information, obtained from past patients who have taken up their abode in various parts of the world, which should be read and considered by any consumptive who contemplates emigrating in search of health or to avoid a further breakdown. It is impossible in a short notice to do justice to the wide range of matters touched on in this useful book, which is full of common sense guided by experience. It should be in the hands of all educated consumptives who, having left the sanatorium with the disease arrested, need to exercise care to ensure against a recrudescence of the disease. It should certainly be read by all who have the charge of sanatoriums for consumptives.

The appearance of a second edition of Dr. SHERMAN G. BONNEY'S *Pulmonary Tuberculosis and its Complications*[7] shows that it has been appreciated, and that the favourable impression of the work which we expressed when the first edition appeared has been endorsed by those for whom the book was written. This edition has been enlarged to the extent of over one hundred and fifty pages by the addition of new matter, whilst the whole work has been revised. Amongst the new chapters is one on Tuberculosis and Traffic which opens a question of considerable practical importance. The almost universal adoption of the sanatorium or open-air treatment of consumptives has led to the erection of sanatoriums in various selected localities where the climatic conditions seem favourable for the cure. There are certain risks in the conveyance of consumptive invalids to and from these places if proper precautions are not taken to prevent infection of the railway carriages or other means of conveyance. The greatest danger would seem to be in the sleeping cars on the railways largely used by consumptives, and Dr. Bonney not only advocates sanitary precautions which he enumerates, but brings forward plans for special compartments in the Pullman cars for the use of consumptive invalids. There is reason to consider that some such precautions might with advantage be instituted, not only in America, but on certain lines which are much used by invalids travelling to health resorts in Europe. The recent advances in our knowledge of vaccine treatment has necessitated the addition of a new chapter on the clinical application of tuberculin and bacterial vaccines.

DR. EPSTEIN has compiled a pocket-book of the diagnosis and treatment of tuberculosis[8] as a guide for the general practitioner. After some general remarks on the requirements, there is a rather long chapter on the early diagnosis of tuberculosis of the lungs, first in adults and secondly in children. This is somewhat disappointing. The various physical signs which may be met with in cases of pulmonary tuberculosis—many of them characteristic of advanced disease—are enumerated and described, but the busy practitioner might reasonably expect the expert to give him a little more practical help from his wider experience to guide him in the diagnosis of the early cases, which generally present considerable difficulties. The section on diagnosis in children is more comprehensive. Other chapters deal with tuberculosis in pregnancy, tuberculosis and marriage, alcohol and tuberculosis, prognosis, duration, and results of tuberculosis. The latter half of the book is taken up with treatment. We doubt whether a compilation such as this, containing a great mass of information obtained from many writers, whose names are given as authority for the various statements for which they are respectively responsible, is of much practical use to the practitioner. What he chiefly wants is the result of the experts' reading and experience condensed and arranged for his guidance rather than a collection of facts and opinions which he must sift, abstract, and appraise for himself.

NOTES ON BOOKS.

THE subject of radium is one that must tempt every author to stray into speculation, but Mr. GUY STEPHEN has placed a severe restraint upon himself in compiling his *Radio-Activity*,[9] and has dealt only with the applications of this substance—or, more properly, this force—to general and specialist practice. Many medical men who, owing to lack of time and opportunity, have been unable to

[6] *Advice to Consumptives—Home Treatment, After-Care, and Prevention.* By Noel Dean Bardswell, M.D. London: Adam and Charles Black. 1910. (Cr. 8vo, pp. 159. 1s. 6d.)

[7] *Pulmonary Tuberculosis and its Complications.* By S. G. Bonney, M.D. Second edition. Philadelphia and London: W. B. Saunders Company. 1910. (Roy. 8vo, pp. 778, 243 illustrations. 30s.)

[8] *Diagnostisch-therapeutisches Taschenbuch der Tuberkulose.* Von Dr. D. Epstein. Berlin and Vienna: Urban and Schwarzenberg. 1910. (Cr. 8vo, pp. 151. M. 5.)

[9] *Radio-Activity; A Brief Guide to its Use in General and Specialist Practice.* Compiled by Guy Stephen, D.P.H.Lond. London: Van Alexander and Company. 1910. (Post 8vo, pp. 44. 1s. 6d. net.)

familiarize themselves with the more or less isolated results of the experts in this field may nevertheless desire to begin to build up a small clinical experience of their own in radium-therapy. In order to assist them the compiler of this pamphlet has analysed the scattered information which is available, levying tribute from the writings of a number of distinguished workers, including such Continental specialists as Wickham, Degrais, and Dominici, as well as British medical men and physicists. There are eight brief chapters, in the course of which we have presented to us an explanation of the physics of radium and radiferous substances; the six or seven different methods by which radio-activity may be turned to therapeutic use; its effect upon normal and pathological tissues; the technique of applicators, dosage, and filtration; the reactions which may be expected; and, finally, the suitability of particular cases for this method of treatment. The last subject is not long-drawn-out, but when, in the experience of a worker who is entitled to speak with authority, radium-therapy has proved of benefit in a given condition, the fact is simply stated. The final chapter deals with the application of radio-active earths, particularly with a by-product which is formed during the process of breaking up uranium ores, and which owes its radiferous quality chiefly to the presence of actinium. The study of mud phenomena has gone on sufficiently long in Paris and elsewhere to make certain conclusions possible, and these the compiler sets down for us. Altogether he considers that, armed with the information which is now available concerning radium in medicine, a medical man may begin to apply the facts in his own practice with not less assurance than that of a newly elected Fellow of a college of surgeons when performing his first independent operation.

It is difficult to be content with even an extended dip into Sir HARRY JOHNSTON'S book on the *Negro in the New World*,[10] which geographically, and in the nature of its contents, covers a very wide field. The opening chapters deal with the negro and the negroid races from an anthropometric point of view, the middle with the history of his treatment by white races in the past and present, while in the concluding chapters the position that men of colour are likely to take up among civilized races at some future time is considered in detail. Sir Harry Johnston's verdict on the negro is decidedly favourable, though his summing up of various points in the negro character conveys the impression of impartiality. The book contains a certain amount of matter of specific interest to epidemiologists, and much of interest to general readers; in addition, it is excellently illustrated. Among other information, one reads with surprise that if the British race can boast of having brought about the end of slavery, it shares with the Dutch the dishonour of first place among nations which have treated negro races badly in the past. It is also indicated that it is in Great Britain more than in any other European country that the negro is now made to feel the disadvantage of his colour.

After a rather flippant introduction by Dr. Oscar Levy and a brief note by the translator, who appears to have done his best, there follow 335 pages of polysyllabic rhodomontade, entitled *On the Tracks of Life*, by Signor L. G. SERA.[11] An alternative title given is "The Immorality of Morality." The book is unutterably tedious, and without any real medical interest.

[10] *The Negro in the New World.* By Sir Harry H. Johnston, K.C.B., D.Sc.Cantab. London: Methuen and Co. Pp. 500.

[11] *On the Tracks of Life.* Translated from the Italian of Leo G. Sera by J. M. Kennedy, with an Introduction by Dr. Oscar Levy. London: John Lane. 1909. (Demy 8vo, pp. 367. 7s. 6d.)

MEDICINAL AND DIETETIC PREPARATIONS.

Lak Cit.

WE have received and examined a sample of "Lak Cit," which is a new dry preparation of milk for infant feeding made by Lak Cit, Limited, and supplied by Messrs. Rocke, Tompsitt, and Co. (4, Red Cross Street, London, E.C.). Our analysis showed this to contain 23.2 per cent. of fat and 24 per cent. of protein. It mixes with hot water in a satisfactory manner, the resulting fluid resembling milk; it differs, however, in the important property that when acid is added it does not give a clotted curd, but the curd forms a finely divided light precipitate. The disadvantage which has to be overcome when cow's milk is used for the feeding of infants is that it forms an indigestible curd in the stomach, whereas human milk does not. This objection is fully met in the preparation under notice, and this advantage will doubtless lead to Lak Cit being extensively employed when mother's milk is not available.

Mango Pulp.

We have received from the Oriental Cannery Company (Honavar, India) samples of their tinned mango pulp, and have submitted it to analysis with the following results:

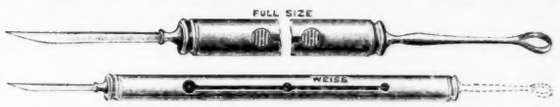
Total solid matter	15.7	per cent.
Sugar (saccharose)	9.1	,,
Fructose and glucose	2.7	,,
Ash	0.6	,,

Salicylic and sulphurous acids, formaldehyde, and other preservatives were tested for and found to be absent. The proportions of the different sugars found are practically identical with those given in published results of the analysis of the pulp of the fresh mango fruit, showing that no addition of sugar has been made, and the material appears to consist of nothing but the pure pulp with its natural juice; the pulp was in an extremely soft condition, the whole forming a uniform semisolid. The flavour was excellent, and the product provides a very satisfactory means of enjoying the mango fruit at any time or place.

MEDICAL AND SURGICAL APPLIANCES.

Combined Tarsal Cyst Knife and Curette.

MR. A. A. BRADBURNE (Southport and Manchester) writes: The removal of tarsal cysts (chalazia) from the eyelids is, on account of the smallness of the hold on the lids and the slipperiness which ensues when bleeding commences, an operation which is not as easy as it looks. The difficulty of it is not lessened by having to change instruments, one to incise and another to scrape out the cyst. To obviate this feature I have devised a combined instrument in which both are contained in a metal tube. The knife blade is wedge-



shaped, thus enabling the cyst to be opened by one cut, and instead of the usual sharp spoon, which gets blocked, and so requires removal, a sharp ring curette is used. Each instrument can be protruded from the tube by the fingers of one hand, and in doing so the other is consequently withdrawn into the tube and cannot injure the eye or the hand of the operator or patient. Messrs. Weiss, of Oxford Street, London, have embodied my ideas in a perfectly satisfactory manner, and it can be obtained from them at a moderate cost.

Operation and Clinical Examination Table.

Mr. H. J. WARING, Surgeon, St. Bartholomew's Hospital, writes: The accompanying illustration shows a new portable form of operation and clinical examination table which I have used for some time, and consider to be a great improvement on the models hitherto existing. It is light, weighing only about 40 lb. complete, very portable, measuring when folded up 36 in. by 20 in. by 5 in., so that it can quite easily be placed inside or on the top of a motor car or carriage, and very strong, being manufactured from oak varnished with hard varnish, and steel, nickel

plated; all joints and working parts are made of metal. It is adjustable to almost any position, and that portion of the table which supports the patient's legs is made to adjust to any angle in order to reduce the pressure on the popliteal spaces to a minimum. The head part of the table can be very easily raised or lowered. It will be found to be a very useful table for surgical or gynaecological operations and clinical examinations, especially when the administration of an anaesthetic is necessary, or when it is wished to use a cystoscope, urethroscope, sigmoidoscope, oesophagoscope, or bronchoscope. Messrs. Allen and Hanburys, Ltd., are the makers.

THE COMPOSITION OF CERTAIN SECRET REMEDIES.*

PREPARATIONS FOR ECZEMA AND OTHER SKIN COMPLAINTS.

WE give below the results of analyses of a further selection of proprietary articles advertised and sold for the cure of eczema and skin affections generally. Some of these are of recent introduction in this country, coming chiefly from America, but are being very largely advertised; others have been established here for a much longer time.

D. D. D.

The preparation supplied under this name is supplied by the D. D. D. Co., Bangor House, Shoe Lane, London, E.C., in bottles at 2s. 3d. and 4s. 6d. per bottle; a 2s. 3d. bottle was found to contain 2.1 fluid ounces.

A press advertisement is headed:

Psoriasis, Dry and Wet ECZEMA, and all skin diseases cured by D. D. D., the new liquid remedy, which gives instant relief, and a positive cure when everything else has failed.

and the following claims are made:

Just a drop or two of D. D. D., the Guaranteed cure for Eczema and all Skin Diseases, applied to the burning skin, and that torturing, endless, nerve-racking itching is at an end. . . . Not only does D. D. D. stop all irritation and pain immediately—we guarantee it—but the cures that invariably follow are permanent—the disease never returns.

The following are extracts from a pamphlet enclosed in the package, entitled "Cause and Cure of Skin Diseases":

D. D. D. is no ordinary patent medicine, but the prescription of the skin specialist, Dr. D. D. Dennis, who used the compound now known as D. D. D. successfully for years on all patients suffering with skin diseases. On urgent advice of his patients, Dr. Dennis was prevailed upon to have the compound put up as a proprietary article for sale through drug stores. It is now manufactured by the D. D. D. Co., London. . . .

Quite a few skin specialists now recommend the D. D. D. Prescription to their patients, and great credit is due to these specialists when they could make so much larger fees by writing out their own prescriptions.

Still, as we find that some doctors prefer to buy D. D. D. themselves and treat their patients with it without telling them what remedy they are using, we cannot, of course, refuse to let such doctors use D. D. D.

The last paragraph does not appear very consistent with the following:

If requested, the formula for D. D. D. prescription will be furnished to medical practitioners for investigation.

In spite of the "guarantee" quoted above that D. D. D. stops all irritation and pain immediately, a caution is given against discouragement:

Occasionally in bringing the disease to the surface, D. D. D. will spread the eruption temporarily over a much larger area. THIS IS NOT A SIGN THAT THE MALADY IS GROWING MORE SERIOUS, BUT ON THE CONTRARY IT SHOWS THAT THE DISEASE IS BEING UP-ROOTED.

It appears that more patience may be required, even when the sufferer is only "in the early stages," than the advertisement quoted above might have led him to suppose:

The person fortunate enough to become familiar with D. D. D. Prescription in the early stages of his disease may commit the error of being impatient of cure. . . . The eruption may have appeared only on a few spots of his body, but this is evidence that the disease germ is lodged in his skin. The germs will grow and multiply and spread; . . . it may take some time to dislodge the germs from his skin. . . . Is it not worth careful study, a few months' patience, to be rid of that horrible malady?

"The germs" are, of course, freely utilized to give an air of scientific knowledge:

The reader familiar with the deeper questions of medical science knows that there is still some dispute among the best authorities on the germ theories, but it is pretty well understood now, largely as a result of Dr. Dennis' discoveries, that eczema is purely and simply a germ disease caused by germs that lodge in the skin. According to the germ theory, there must be several species of eczema germs, causing the several diseases known as eczema, psoriasis, ring worm, dandruff, etc. The germs are all of the same type and produce ailments with varying symptoms, all eczematous. The germs live only in the skin, feeding on the oily ingredients of the epidermis.

It might be interesting to know where "Dr. Dennis' discoveries" were published.

D. D. D. is put up in two forms, called "D. D. D. Ordinary" and "D. D. D. Strong"; the "Ordinary" was taken for analysis. The directions for this are:

The remedy may be applied in various ways; it is best, however, to rub the skin as little as possible while making the application, hence we recommend it to be applied by saturating a piece of absorbent cotton with D. D. D. and dabbing this gently on the parts affected, or applying it with a small camel hair brush, so that the pores will absorb the remedy.

Analysis showed the liquid to be composed as follows:

Salicylic acid	0.75 part
Phenol	1.18 parts
Methyl salicylate (oil of wintergreen)	1.00 part
Glycerin	9.28 parts
Alcohol	65.10 parts by measure
Water, to ...	... 100 parts by measure.

It had a slight yellow colour, which may have been due to a trace of colouring matter, or may have been developed from the oil of wintergreen.

The alcohol is by far the most costly ingredient, and the estimated cost of ingredients for 2.1 fluid ounces is 3¾d.

HOMOCEA.

This preparation is made by Homocea, Limited, Willesden Junction, London, N.W. A 1s. 1½d. tin contained about ¾ ounce, and a 2s. 9d. tin about 2½ ounces. It is recommended for a very large number of disorders, as is shown by the following extracts from a circular enclosed in the package:

Homocea . . . Ointment . . . is far and away the best remedy known for Open Wounds, Sores, Cuts, Ulcers, Bad Legs, Burns, Scalds, Chaps, Wasp, Bee, and Insect Stings, Earache, Inflamed Spots, Skin or Flesh troubles of all sorts, Broken Chilblains, &c. In treating any of the above lay Homocea lightly with the finger, or spread some on clean rags or lint and apply to the parts affected. For Bruises, Blotches, Chaps, Chilblains (unbroken), Eruptions, Swellings, Mumps, Croup, Sore Eyes, Rashes, Ringworm, Jelly Fish Stings, Sunburns, &c., &c., smear or gently rub in Homocea where required.

For Piles (bleeding or blind) Homocea stands alone amongst remedies. It affords immediate relief at all times, and in many cases absolutely cures.

For Internal Piles the insertion of Homocea Suppositories is recommended.

Cold in the Head is cured by putting Homocea up the nostrils and rubbing the bridge of the nose well with the Ointment at night.

Soft Corns and Bunions are cured by applying Homocea as a pad to be kept on day and night.

Eczema (in many forms), Hay Fever, Prickly Heat Jigger, and Veldt Sores, Warts, &c., &c., cured by Homocea.

Homocea will cure the Bites and Stings of Mosquito, Gnats, Sand Flies, and, better still, will prevent these pests from attacking you, if the face, neck, hands, and other exposed parts of the body are smeared with the Ointment.

It is impossible to bring within the compass of a short circular all that Homocea in its various forms will do, but we claim, without fear of contradiction, that there are few of the ordinary every-day ailments incidental to the human frame that Homocea, if promptly and properly applied, will not immediately alleviate and cure.

The efficacy of Homocea is easily and unanswerably explained. It kills inflammation immediately it comes in contact with it, and you can readily understand why pain vanishes and wounds heal so quickly.

Analysis showed the presence of eucalyptus oil in considerable proportion, with beeswax, a soft fat, and a very small quantity of ammonia; evidence was also obtained of a small proportion of another essential oil, apparently oil of lemon, though, as essential oils are complex in composition, it is not possible to speak positively on this point. Other medicinal ingredients were found to be absent. The quantities of the ingredients found were determined as accurately as possible, and the results gave the following formula:

Oil of eucalyptus	25 per cent.
Oil of lemon	0.2 ,,
Beeswax...	25 ,,
Fat	49 ,,
Solution of ammonia	0.8 ,,

* Previous articles of this series were published in the following issues of the BRITISH MEDICAL JOURNAL: 1904, vol. ii. p. 1585; 1906, vol. ii, pp. 27, 1645; 1907, vol. i, p. 213; vol. ii, pp. 24, 160, 209, 393, 530, 1653; 1908, vol. i, pp. 833, 942, 1373; vol. ii pp. 86, 505, 1022, 1110, 1193, 1285, 1566, 1697, 1875; 1909, vol. i, pp. 31, 909, 1128; vol. ii, p. 1419; 1910, vol. i, pp. 151, 213, 393, 1005, 1063, 1120; vol. ii, p. 982.

The characters of the fat agreed with those of a mixture of lard and cocoanut oil in about equal parts. An ointment prepared according to this formula was practically undistinguishable from the original.

Estimated cost of ingredients for 2½ ounces (2s. 9d. tin), 2½d.

ECZOLINE TREATMENT.

This "treatment" is supplied by W. W. Hunter, Swindon, and consists of ointment, tablets, and soap; the two former were taken for examination.

An advertisement contains the following somewhat disconnected expressions:

A wonderful Remedy. Eczema, Rashes, Pimples, Spots, Sores, Ringworm, Chaps, Chilblains, Rough Skin, Bad Legs. Stops all irritation at once. Cured by the Eczoline Treatment. Worth its weight in gold.

Eczoline Ointment.—**A 1s. 1½d. box contained about 1 ounce. On the box it is described as**

A positive cure for Eczema and good for all skin diseases.

Analysis showed the presence of zinc oxide, sulphur (flowers), glycerin, a little water, and a fat which agreed well in its characters with lard, with a trace of oil of lemon; no other constituents were found. The quantities of the respective ingredients were determined, and the results gave the following formula:

Flowers of sulphur	39	per cent.
Zinc oxide	3 7	,,
Glycerin	13 5	,,
Lard	39.8	,,
Water	4	,,
Oil of lemon	a trace	

Estimated cost of ingredients for 1 ounce, ½d.

Eczoline Tablets.—**A 1s. 1½d. box contained 43 sugar-coated tablets, having an average weight, without the coating, of 5·2 grains each. These are described on the label as**

For Cooling, Purifying, and Strengthening the Blood. Dose—One or two Tablets three times a day after meals.

Analysis showed the tablets, from which the coating had been removed, to consist of

Ferrous sulphate	16 5	per cent.
Sulphur (precipitated)	56	,,
Talc	3 4	,,
Starch	7 3	,,
Extractive	16 8	,,

The extractive was bitter, and showed the reactions of extract of cascara sagrada, but it was not bitter enough or dark enough to consist only of this extract, and appeared to be a mixture of extract of cascara sagrada, and an inert extract, the former constituting about 5 per cent. of the substance of the tablets.

Taking the whole of the extract as being of the same price as extract of cascara sagrada, the estimated cost of the ingredients for 43 tablets is ½d.

DR. CASSELL'S MEDICINES.

These are supplied by Dr. Cassell's Medicine Co., Ltd., King Street West, Manchester. This firm is described on its notepaper as "Sole Agents for Great Britain, India, Africa, Australia, and all European Countries for Dr. Cassell's Specialities," and the "Specialities" appear to be really of American origin. The form of advertisement which appears to be most in favour is a small announcement of

An indispensable illustrated book written by an expert, dealing in a clear, commonsense way with a safe and speedy home cure of Skin Diseases of all kinds, with special chapters on Diseased Blood, Ulcers, Bad Legs, and "The Woman Beautiful,"

to be sent free on application. A request for a copy of this brought a pamphlet of 31 pages, together with a "self-examination form," and a letter urging that the form should be filled up and returned. In a further circular the following statement is made with regard to the mode of procedure:

When you consult a doctor he first enquires into the causes leading up to your trouble, then carefully examines all the symptoms and indications. Upon the results he forms his diagnosis which in turn suggests the treatment.

We do exactly the same when you return the examination form filled up; we carefully examine it and from the answers you give to the questions we are able at once to select for you, from the numerous Ointments, Powders, Mixtures, and Medicated Tablets manufactured for this Company from the formulas of that world-famed specialist, Dr. Cassell, a treatment perfect in every detail with the requirements of your case.

It is further stated that:

There is not one among the many healing extracts used which is not highly lauded by skin specialists the world over.

Which seems rather inconsistent with the statement on the next page that:

Several of the medicaments used in the Ointments and Blood Tablets are very rare and not in use in Great Britain and do not form any part of the British doctors' medical outfit, yet are of such incalculable value as to constitute the basic strength and great healing virtue of the treatment.

The "self-examination form" was as follows; the words in italics are the answers which were filled in:

QUESTIONS.

Date 19

Name in full.....

Address in full.....

Age. *37.* Sex. *Male.*
Occupation. *Groom-Gardener.* Stout or thin? *Medium.*
Married or single? *Single.* How long suffered? *About 2 years.*
Are you able to work? *Yes.* If not, how long have you been disabled?
What are you suffering from? *Eczema.*
What was your previous health? *Good.*
Where are you affected? *Arms worst; also neck and shoulders.*
What do doctors call your trouble? *I don't know.*
Have you been treated for it? *Not by doctors.* If so, how? *I have used some lotions.*
Have you attended any hospital? *No.*
Is your general health good? *Yes.*
Are you constipated? *No.*
Have you indigestion? *No.*
Do you stand much? *Yes.*
Are you in contact with much heat? *No.*
Do you perspire freely? *Yes.*
Are your hands much in soap and water? *No.*
Do you use much soda? *No.*
Does your employment entail contact with gritty or irritating material, and if so, what? *No.*
Have you much itching? *Yes.*
Have you a scaly eruption? *No.*
Is the part red or inflamed? *Yes.*
Is there any discharge? *Sometimes.* If so, what colour is it, and does it smell? *Like water.*
Have you any bleeding from the part? *No.*
Have you any pain? *Yes.*
Is your trouble inclined to spread? *Yes.*
Do you suffer from varicose veins? *No.*
Do little blisters form? *No.*
Did the trouble start with a pimple? *No.*
Have you any rash? *No.* If so, where? *No.*
Have you a sore throat? *No.*
Have you recently had measles or any fever? *No.*
Have you ever suffered from any specific blood disease? *No.* If so, what?
What is your usual diet? *Porridge, bread and butter, coffee, meat and potatoes, beer, cheese.*
Do you eat much oatmeal, shellfish, or tinned food? *A good deal of oatmeal porridge.*
Are you a teetotaler? *No, but very moderate.*
Are you worse more at one season than another? *No.*
What is your own opinion as to the cause? *I don't know.*
Here fill in any other details you may think of use.

As the question, "What are you suffering from?" was answered by the word "eczema," it was not altogether surprising to be told in the letter which was received in reply:

Your Examination Form has been duly received and carefully studied. You are suffering from a severe and disagreeable form of eczema.

The letter proceeds with a warning that it is

inadvisable to use ordinary salves, lotions, etc., indiscriminately, as you will readily realize that what will suit one person will not necessarily suit another, and many doctors don't appear to reconize [*sic*] the fact that the complaint should be treated both locally and constitutionally, therefore their treatment often proves ineffective. We make a special point of treating every case entirely upon its merits, and yours is one in which Dr. Cassell's combined treatment will produce speediest and most brilliant results.

It appears, however, that even these "speediest and most brilliant results" are not very speedy, as a further paragraph states:

Although it may take a few months to effect a permanent cure, we are quite confident, if you make up your mind to have yourself properly treated now, it will save much suffering and expense in the future.

The letter further intimated that the treatment would consist of a dusting powder, tablets to be taken internally, and an ointment, and that the charge would be 12s. 6d.

As no reply was sent at once, this was followed about a week later with another letter, offering to send the medicines for 9s. 6d., and containing the statement:

We are quite safe in guaranteeing a cure in your case provided you make up your mind to persevere for a reasonable length of time.

The sum required was then sent, and the articles named were received. The powder, in a pill-box, weighed a little less than 1 ounce; the bottle of tablets contained 90, of average weight 6 grains; and the pot of ointment contained just over 1 ounce. They were accompanied by an intimation that a further supply would cost 9s. 6d., and the following directions:

Ointment. Wash the hands thoroughly clean in hot water and apply a little of the Ointment to the affected parts every night on retiring. It is not necessary to use a great quantity, the chief thing being to have it evenly distributed on the parts requiring treatment.
Powder. A small portion should be dusted on with a piece of clean lint every morning on arising.
Tablets. One to be swallowed whole in water three times daily, between meals.

Cassell's Dusting Powder.—This was labelled "Antiseptic Dusting Powder No. 2." Analysis showed it to contain boric acid, talc, maize starch, and powdered slippery elm bark. The proportions of the first two ingredients were determined and the other two estimated as accurately as possible, and the formula given below was obtained. A powder prepared in accordance with this formula agreed in all respects with the original, except that the latter had a faint perfume and a creamy tint; the traces of colouring matter and perfume were far too minute to be identified.

Powdered talc	... ...	60 per cent.
,, boric acid	... ...	20 ,,
,, maize starch	... ...	17 ,,
,, slippery elm bark	...	3 ,,

Estimated cost of ingredients for 1 ounce, ⅓d.

Cassell's Blood Cleansing Tablets.—The label was marked "Price 4s. 6d. a bottle," and, as stated above, the bottle contained 90 tablets of about 6 grains each. Analysis showed the tablets to contain:

Phenolphthalein	... ...	0.75 per cent.
Potassium iodide	... ...	1.25 ,,
Sugar	... ...	81 ,,
Talc, approximately	... ...	11 ,,
Calcium carbonate and sulphate, approximately	... ...	2 ,,
Water	... ...	1 ,,
Extractive	... ...	3 ,,

The extractive showed no distinctive characters by which it could be identified; the tablets had a faint anise-like odour; no alkaloid or other active principle was found. The dose of phenolphthalein in one tablet is 0.045 grain, the usual dose being 1 to 8 grains; the dose of potassium iodide in one tablet is 0.075 grain, the official dose being 5 to 20 grains.

If the extract present is taken as being of the same price as extract of taraxacum, the estimated cost of the ingredients for 90 tablets is ½d.

Cassell's Ointment.—This was labelled "Ointment No. 2," and the pot contained just over 1 ounce. Analysis showed it to contain oils of wintergreen and eucalyptus, lanoline, a fat or oil, soft paraffin, boric acid, a small quantity of alkali, and a powdered vegetable tissue. The latter appeared to consist chiefly of a bark; it was carefully compared with a number of powdered drugs, but was not found to agree with any drug in ordinary use; it showed a good deal of resemblance to powdered krameria root, and agreed exactly with it in the colour changes caused by various reagents, and while not identical with either of the species of krameria compared with it, the resemblance suggested that it might be from a nearly related plant. The proportions of the different constituents were determined as closely as possible, and the following formula was arrived at:

Boric acid	... ...	8 per cent.
Borax	... ...	2 ,,
Oil of eucalyptus	... ...	2 ,,
Oil of wintergreen	... ...	3 ,,
Anhydrous lanoline	... ...	4 ,,
Oil (? olive)	... ...	8 ,,
Soft paraffin	... ...	63 ,,
Powdered drug	... ...	7 ,,
Water ...	... ...	3 ,,

An ointment prepared in accordance with this formula, with krameria root as the drug, closely resembled the original.

SULPHOLINE LOTION.

This lotion is prepared by John Pepper and Co., Ltd., Bedford Laboratory, London, S.E. A 1s. bottle contained 2 fluid ounces. This preparation is advertised in the following terms:

Eczema and all skin ailments are completely cured by Sulpholine Lotion, which directly attacks all forms of Eczema, Eruptions, Pimples, Acne, Spots, Blackheads, or Disfiguring Rashes, leaving the skin in the soft, clear, supple, attractive condition designed by Nature.

And in a pamphlet enclosed in the package it is further stated that

Sulpholine Lotion is the only external skin remedy.
There is no phase of Eczema, chronic or acute, red, dry, scaly or pustular (moist), psoriasis, pityriasis, impetigo, prickly heat, tetter, salt-rheum, or even superficial scorbutic ulcerations, but Sulpholine will grapple with, and in most cases completely eradicate, leaving the skin its natural, clear, smooth, pliable surface.

The directions on the label are:

To be damped on with a handkerchief or piece of cotton wool, and allowed to dry. One application of the Lotion should be at bedtime.

Analysis showed it to contain:

Sulphur, precipitated	... 3 parts
Zinc oxide ...	... 2.1 ,,
Calcium sulphate	... 0.6 part
Glycerin	... 9 parts
Strong rose water, to	... 100 parts by measure

The rose water is by far the most expensive ingredient, and the estimated cost of ingredients for 2 fluid ounces is 1¼d.

CADUM.

Supplied by Omega, Limited. A 1s. 1½d. tin contained just over 1 oz.

This is described in a circular enclosed in the package as:

The New Medical Discovery for the Treatment of Eczema, Psoriasis, Ringworm, Acne, Pimples, Blackheads, Itch, Facial Eruptions, Salt Rheum, Shingles, Herpes, Hives, Prickly Heat, Boils, Rash, Itching Piles, Cuts, Sores, Burns, Chafing, Sunburn, Scabies, and other Skin Troubles.

Its origin is thus described:

The discovery of Cadum, the wonderful new skin remedy, was not the result of mere chance or accident, but of patient, painstaking investigations, extending over a long period, by an eminent chemist who became possessed of the conviction that the old method of taking internal remedies to cure external diseases was erroneous. After a careful study of all the best-known products, it was discovered that an extract from a particular part of a well-known tree possessed superior antiseptic and curative properties, and exerted a powerful, beneficial effect on the skin tissues. It was, however, found that this valuable vegetable extract possessing such desirable qualities was difficult to compound with other ingredients necessary to present it in a form convenient to use. After further investigations and experiments, a method of special treatment was devised by which this seemingly insurmountable difficulty was overcome, resulting in a unique combination of ingredients, possessing to a marked degree valuable therapeutic properties, the like of which has never before been produced.

Directions are given for the use of this preparation in some thirty affections of the skin.

Analysis showed the presence of zinc oxide, sulphur, boric and salicylic acids, oil of cade, and a paraffin basis. The proportions of the several ingredients were determined as accurately as possible; since oil of cade is variable in composition, and there is no direct means of determining its quantity in such a

mixture, this was estimated from a number of comparisons with known mixtures, and the figure given is certainly not far from exact. The formula arrived at is:

Zinc oxide	11.3	per cent.
Flowers of sulphur	8.0	,,
Boric acid	3.1	,,
Salicylic acid	0.8	,,
Oil of cade	7	,,
Hard paraffin	10	,,
Soft paraffin	60	,,

Estimated cost of ingredients for 1 ounce, ½d.

POSLAM.

This is supplied by the Emergency Laboratories, New York. A 2s. 3d. tin contained ⅔ ounce.

It is described in a circular enclosed in the package as

The Newest Medical Discovery for the treatment of Eczema and all other skin affections.

Some further extracts from this circular are as follows:

It is a singular fact that, notwithstanding the great strides that have been made in medicine for the past decade or two, skin diseases, and particularly eczema—the worst of them—are about as little understood by the medical profession to-day as they were a generation back.

Eczema is one of the most prevalent diseases, and statistics show that at least 50 *per cent. of humanity have it in some form or other.* It may be that physicians have failed to give this trouble the proper amount of study because, not being a fatal disease, they have considered it unimportant enough to wilfully neglect its thorough diagnosis.

Another reason may be that the *advertised preparations for eczema have been so exploited as to convey wrong impressions about the disease*, ascribing it to causes most ridiculous, and harping on the erroneous idea that it is a blood trouble, only curable by internal remedies. Doctors, in the absence of any recognized specific for the disease, have given either internal remedies or ordered the use of special soaps, both of which courses show that the average medical man is helpless to combat the growing trouble of skin diseases. . . .

LOTIONS AND INEFFECTUAL "REMEDIES."—*The potent ingredients of a remedy, which are necessary to the successful cure of eczema, cannot be contained in the form of a liquid. Lotions, besides containing mercury, must necessarily contain some alcohol or water, which are irritants and should be rigidly avoided. . . .*

There is no need of an extended course of treatment in the cure of eczema. If the remedy is the right one, its effects will be apparent immediately, and the progress of its work may be noted daily. . . .

POSLAM, THE CURE.—*The success of Poslam in the cure of eczema and all kindred skin diseases, has been absolute since its first introduction to the public and the medical profession. It is entirely different from anything yet used, and as a remedial agent in skin diseases, may be said to be positively infallible in its action.* Hitherto there has been a common base to all ointments and salves that were supposed to be "remedies" for eczema. *Poslam has a base of its own, and contains no grease, poisons, mercury, or any other harmful constituent.*

The general directions are:

Apply Poslam twice daily, night and morning, directly to the parts affected.

Do not rub or irritate, but apply gently.

Analysis showed the presence of zinc oxide, sulphur, starch, salicylic acid, oil of birch tar, oil of cade, lanoline, and soft paraffin. Since oil of birch tar and oil of cade are both very complex bodies of variable composition, it is not possible to determine exactly the quantities present in such a mixture; it is even possible that some samples of oil of birch tar might possess the properties observed, without the presence of oil of cade, and the latter is given in the formula with this reservation. The various ingredients were determined or estimated as exactly as possible, and the following formula was arrived at:

Zinc oxide	12	per cent.
Flowers of sulphur	8	,,
Maize starch	18	,,
Salicylic acid	1.5	,,
Oil of cade	1.5	,,
Oil of birch tar	8	,,
Anhydrous lanoline	25.5	,,
Soft paraffin	25.5	,,

Estimated cost of ingredients of ⅔ ounce, ½d.

POOR LAW MEDICAL OFFICERS' ASSOCIATION OF ENGLAND AND WALES.

A Council meeting of the Poor Law Medical Officers' Association of England and Wales was held at 34, Copthall Avenue, E.C., on Tuesday, October 18th. Dr. D. B. Balding was in the chair.

A letter was considered from a country member complaining that the guardians in his district had appointed as district medical officer a practitioner who resided seven miles away from his district. This had been sanctioned for one year by the Local Government Board, but recently he had been re-elected by the guardians for a further period of three years. It was unanimously considered by the Council that the matter required investigation; that on the face of it it was a scandal to appoint a district medical officer living seven miles away from his district, having regard to the necessities of the sick poor. The honorary secretary was instructed to make further inquiries into the matter.

A communication was received from a public vaccinator complaining that, because he had resigned his district medical officership on account of inadequate remuneration, he was now threatened with the loss of his vaccination office. The local Division of the British Medical Association had banned the appointment he had resigned, and so far there had been no applicants; but he felt sure that if no one would take this post at the old rate the guardians would endeavour to attract applicants by adding the post of public vaccinator to it. Strong comments were made by those present on the unprotected position of the public vaccinator, and the unscrupulous use made by the guardians of the remuneration from this source to underpay Poor Law appointments. It was regretted that no assistance could be given to the Council's correspondent. Under the present law only a month's notice need be given by the guardians to determine any public vaccinator's office, and this could be done without any reason, and it appeared that the Local Government Board had no control in the matter at all.

Another correspondent wrote complaining of the refusal of his guardians to allow him the extra fee of £2 for attendance on a long case of phlegmasia dolens, on the ground that he had not attended at the confinement of the patient, having only received the order twelve days afterwards. He quoted from Article 182 of the General Order of July, 1847, which lays down that a district medical officer shall be entitled to a fee of £2 in any "special case where great difficulty may have occurred in the delivery, *or long subsequent attendance in respect of some puerperal malady or affection may have been requisite.*" There was some difference of opinion on this point. It was thought by some that in order to be entitled to this fee the medical officer must have been called to the confinement, or very shortly afterwards. But a majority of the Council thought that under the article any long-continued attendance on a puerperal case was sufficient, and the honorary secretary was instructed to write and advise the member to push his claim and proffer the assistance of the association.

A delegate from the district medical officers of a metropolitan borough also attended the Council meeting to ask for assistance. He stated that the guardians, owing to a recent resignation of one of the district medical officers, were endeavouring to reduce the number of district medical officers from five to four, and to force on to two of their number the vacated district under very inequitable conditions. The district medical officers claimed that there should be a rearrangement of the whole area; that, under the present arrangement, only one of their number received about the average payment of the district medical officers throughout the metropolitan district—namely, 3s. per order; that all the others were being paid less than 2s. per order. They had sent in a memorial to their board of guardians, and if this were ignored they proposed to carry it to the Local Government Board. The Council expressed its sympathy, and promised all the assistance in its power.

The Honorary Secretary read a letter from Mr. Shirley Fussell, asking the association to appoint four delegates to meet delegates from the National Poor Law Officers' Association, the Union and Rural District Clerks' Association, and the Workhouse Masters and Matrons' Association in a joint conference to consider the recommendations of

the Royal Commission on the Poor Laws affecting the interests of Poor Law officers, and report as to what action (if any) it is necessary or desirable to take now or in the future to safeguard the interests of the service. The Council appointed Dr. Holder (Hull), Dr. Napper (Cranleigh), Dr. Toogood (London), and Dr. Major Greenwood (London) to represent the Poor Law Medical Officers' Association in the proposed conference.

WORKMEN'S COMPENSATION STATISTICS FOR 1909.

THE Home Office Report on Statistics of Compensation and of Proceedings under the Workmen's Compensation Act, 1906, and the Employers' Liability Act, 1880, during the year 1909,[1] has just been issued. This Blue Book provides interesting reading, the facts and figures being simply presented and readily followed and understood.

Full returns from seven great industries—mines, quarries, railways, factories, harbours and docks, constructional works, shipping—are under consideration, and include over 6½ millions of persons employed within the provisions of the Compensation Act. In these industries compensation was paid in 3,341 cases of death and in 332,612 cases of disablement. The average payment in death claims was £154, and in cases of disablement £5 6s.

Taking the several groups together, the annual charge for compensation was about 6s. 10d. per person; it ranged from 3s. 5d. per person in factories to 20s. 1d. in mines. In the coal-mining industry the charge under the Act amounts to about 0.8 of a penny per ton of coal raised. The total amount of compensation paid during the year in all industries appears to be something just under three millions.

With regard to the twenty-four kinds of industrial diseases included in the Act, compensation was paid in 33 cases of death and in 3,313 cases of disablement, 596 being continued from the previous year, leaving 2,717 in which the first payment was made during the year. In the mining industry there occurred 2,290 of these cases, nystagmus cases being 631; ankylostomiasis, 8; beat hand, 570; beat knee, 880; beat elbow, 63; inflammation of synoviae of wrist-joint and tendon sheaths, 128; lead poisoning, 351; anthrax, 22; arsenic, 4; other occupations showing cases of chrome poisoning, 23; compressed air illness, 2; phosphorus poisoning, 1; carbon bisulphide, 1; eczematous ulceration of the skin, 41; epitheliomatous cancer, 3; poisoning by nitro and amido derivatives of benzine, 8. There were no cases of poisoning by nitrous fumes, nickel carbonyl, or African boxwood; no cases of glanders, cataract in glassworkers, or telegraphists' cramp.

The number of cases dealt with in England and Wales by county court judges and county court arbitrators under the Act of 1906 was 4,390, of which 4,201 cases were decided by judges, 42 by special arbitrators; 147 settled by acceptance of money paid into court; in 1908 the figures were 3,795, 3,602, 47, and 146 respectively. In addition to the cases dealt with by judges and arbitrators, there were 1,798 cases which were either withdrawn, settled out of court, or otherwise disposed of in such a manner as to afford no definite evidence to the court officials as to the result, making a total of cases taken into court of 6,188.

With regard to some outstanding cases under earlier Acts, 244 of these were decided by judges, one by a special arbitrator, 76 otherwise disposed of—a total of 321 taken into court.

Of claims for compensation under the Act of 1906, finally settled in 1909 within the cognizance of the courts, the decision in 2,427 cases was in favour of the applicant, and 660 in favour of the respondent. The proportion of cases in which the applicant was successful was 79 per cent. as against 82 in 1908, 84 in 1907, 83 in 1906. In the cases under the Act of 1906 in which payment was awarded, the average compensation for total incapacity was 12s.; for partial incapacity, 9s. 6d. Under the Employers' Liability Act, of the 67 cases in which judgement was given for the plaintiff, there were only 3 fatal cases, the total compensation awarded being £489; 40 were cases of total incapacity, and in these the average award was £79 per case; 24 were cases of partial incapacity, the average award being £56 per case.

The average amount of solicitors' costs was £25 18s. under Employers' Liability Act, and £11 17s. 10s. under the Compensation Acts. During the year, the county courts in which there were the greatest number of arbitrations under the Compensation Acts were Liverpool, 398; Bow, 243; Manchester, 166; Pontypridd, 141; Salford, 122; Tredegar, 144; Wigan, 112; Leeds, 107; Cardiff, 102. There still exists a higher proportion of cases under the Employers' Liability Act in the metropolis. There were only eight courts in which there was no case either under the Employers' Liability or Workmen's Compensation Acts, as against 204 in 1904, 193 in 1905, 203 in 1906, 120 in 1907, 14 in 1908.

The number of cases going to the Court of Appeal in England was 135, or just over 2 per cent. of the cases that came before the county courts; 68 appeals were by workmen, and 67 by employers. Of the former, 23, and of the latter, 23 were successful; 25 were abandoned, withdrawn, or settled before the hearing. In a list of appeal cases, out of 110 in which the point on which the appeal arose is known 33 turned on the meaning of the words: "Accident arising out of, and in the course of, the employment."

There were two appeals from the Court of Appeal to the House of Lords, the workman being the appellant in one case. Neither was successful. The number of medical referees appointed was, on December 31st, 1909: England, 223; Scotland, 82; Ireland, 45. The number of cases referred to them by a judge, arbitrator, or committee was 297.

The number of cases in which a certificate was given in cases referred on the joint request of both workman and employer was 300. The cases in which a workman desired to receive his compensation abroad, and so required a certificate, were only 3. Medical referees sat as assessors under Schedule II (5) of the Acts in 376 cases. It is to be noted that, as in previous years, the great majority of claims were settled by agreement.

[1] Printed for His Majesty's Stationery Office by Darling and Son, Limited, 34-40, Bacon Street, E. To be purchased directly or through a bookseller from Wyman and Sons; Oliver and Boyd; E. Ponsonby. Price 7d.

AUSTRALASIAN MEDICAL CONGRESS.

THE ninth session of the Australasian Medical Congress will be held in Sydney during September, 1911, under the presidency of Mr. F. Antill Pockley, M.B., C.M.Edin., M.B.Syd., M.R.C.S.Eng. The session will commence on Monday, September 18th, and end on Saturday, September 23rd. A social function will be held on the Saturday preceding. The Senate of the University of Sydney has granted the use of its buildings and grounds. The Government of New South Wales has given its countenance and support to the congress. Addresses will be given in full congress by the presidents of certain sections, to be decided upon. Special meetings of congress will be devoted to consideration of special subjects, which have still to be selected.

Patrons.

The following are patrons of the Congress: His Excellency the Right Honourable the Earl of Dudley, P.C., G.C.M.G., G.C.V.O., Governor-General and Commander-in-Chief of the Commonwealth of Australia; His Excellency the Lord Chelmsford, K.C.M.G., Governor of New South Wales; His Excellency Vice-Admiral Sir Richard Poore, Bart., K.C.B., C.V.O., Naval Commander-in-Chief of the Australasian Station; His Excellency the Lord Islington, D.S.O., Governor of the Dominion of New Zealand; His Excellency Sir Thomas D. Gibson-Carmichael, Bart., K.C.M.G., Governor of Victoria; His Excellency Sir William MacGregor, G.C.M.G., C.B., M.D., Governor of Queensland; His Excellency Admiral Sir Day Bosanquet, G.C.V.O., K.C.B., Governor of South Australia; His Excellency Sir Gerald Strickland, K.C.M.G., Governor of Western Australia; His Excellency Major-General Sir Harry Barron, K.C.M.G., C.V.O., Governor of Tasmania; His Excellency Sir Everard F. Im Thurm, K.C.M.G., Governor of Fiji, High Commissioner of the Pacific Islands His Excellency the

Honourable J. H. P. Murray, Lieutenant-Governor and Chief Judicial Officer of New Guinea; the Honourable W. P. Cullen, LL.D., Chief Justice of New South Wales; The Lord Plunket, K.C.M.G., K.C.V.O., late Governor of New Zealand; Admiral Sir Harry Rawson, G.C.B., G.C.M.G., late Governor of New South Wales; the Honourable Sir Normand MacLaurin, M.A., M.D.Edin., LL.D., M.L.C., Chancellor of the University of Sydney.

Officers.

The following are the officers of the Congress: *President:* F. Antill Pockley, M.B., C.M.Edin., M.B.Syd., M.R.C.S. Eng., Sydney. *Treasurer:* S. H. MacCulloch, M.B., C.M.Edin., Sydney. *General Secretary:* Arthur Palmer, M.B., C.M, F.R.C.S.Edin., Elizabeth Bay. *Associate Secretary:* A. J. Brady, LL.M., K.C.P., L.R.C.S.Irel., Sydney.

Vice-Presidents.

J. C. Verco, M.D.Lond., F.R.C.S.Eng. (President, Adelaide Congress, 1887); Sir P. Sydney Jones, M.D. Lond., F.R.C.S.Eng. (President, Sydney Congress, 1892); F. C. Batchelor, M.D.Durh., M.R.C.S.Eng., L.R.C.P.Edin. (President, Dunedin Congress, 1896); Hon. G. H. Butler, M.R.C.S.Eng., L.R.C.P.Lond. (President, Hobart Congress, 1902); Professor E. C. Stirling, C.M.G., M.A., M.D Cantab., F.R.C.S.Eng., F.R.S., LL.D. (President, Adelaide Congress, 1905): Professor H. B. Allen, M.D.Melb. (President, Melbourne Congress, 1908); Surgeon-General W. D. C. Williams, C.B., M.R.C.S., L.R.C.P.Lond., Director-General of the Army Medical Services of the Commonwealth, Melbourne; Fleet Surgeon V. Gunson Thorpe, M.R.C.S.Eng., L.S.A.Lond., Senior Naval Medical Officer, Australasian Station; Fleet Surgeon F. Falconer Hall, M.B., C.M., A.M., late Senior Naval Medical Officer, Australasian Station.

Sections.

The work of the Congress will be divided among twelve sections: 1. Section of Medicine: *President*, W. T. Hayward, M.R.C.S., L.R.C.P.Irel. (Adelaide). 2. Section of Surgery: *President*, G. A. Syme, M.B., M.S.Melb. (Melbourne). 3. Section of Obstetrics and Gynaecology: *President*, G. Rothwell Adam, M.D.Melb., C.M.Edin. (Melbourne.) 4. Section of Anatomy, Physiology, and Pharmacology: *President*, E. H. Embley, M.D., Ch.B.Melb. (Melbourne). 5. Section of Pathology, Bacteriology, and Tropical Medicine: *President*, Wilton W. Love, M.B., C.M.Edin. (Brisbane). 6. Section of Public Health and State Medicine; *President*, James W. Hope, F.R.C.S.Edin., L.S.A., D.P.H., R.C.P. and S.Irel. (Fremantle). 7. Section of Ophthalmology; *President*, J. W. Barrett, M.D., M.S.Melb. (Melbourne). 8. Section of Psychological Medicine and Neurology; *President*, W. Beattie Smith, F.R.C.S.Edin. (Melbourne). 9. Section of Diseases of Children: *President*, A. Jeffreys Wood, M.D., B.S.Melb. (Melbourne). 10. Section of Naval and Military Medicine and Surgery; *President*, Colonel C. S. Ryan, M.B., C.M.Edin. (Melbourne). 11. Section of Dermatology, Radiology, and Medical Electricity; *Presidents*, H. F. Lawrence, M.R.C.P.Edin., F. J. Clendinnen, M.D.Brux., L.R.C.P.Lond. (Melbourne). 12. Section of Laryngology, Rhinology, and Otology; *President*, W. L. Robertson, M.B., C.M.Edin. (Brisbane).

State Secretaries of Congress.

The following are the State Secretaries of the Congress:—Victoria: Arthur Jeffreys Wood, M.D., B.S.Melb. (Melbourne). Queensland: Wilton W. Love, M.B., C.M.Edin. (Brisbane). South Australia: John B. Gunson, M.B.Adel., M.R.C.S.Eng. (Adelaide). Tasmania: Gregory Sprott, M.D., C.M., D.P.H.Glasg. (Hobart). Western Australia: A. J. H. Saw, M.A., M.B., B.S., M.D., F.R.C.S.Edin. (Western Australia). New Zealand (South Island): Stanley Batchelor, F.R.C.S.Eng., L.R.C.P.Lond. (Dunedin); (North Island) W. E. Herbert, M.B., C.M., M.D.Edin., F.R.C.S. Edin. (Wellington).

DR. T. L. LLEWELLYN, of Bargoed, Wales, has been awarded the studentship of the Royal Society founded by the late Professor Tyndall for scientific research on subjects tending to improve the conditions to which miners are subject. Dr. Llewellyn proposes to investigate the cause and cure of miner's nystagmus.

SCIENCE NOTES.

THE products of the dry distillation of pure cellulose do not appear to have been studied hitherto in any great detail. In view of the fact that the exact chemical constitution of cellulose is still a matter of dubiety, it is not impossible that a more accurate knowledge of these volatile products might afford some important information, and this consideration has induced Erdmann and Schaefer to undertake an investigation along this line. Their results are published in the *Berichte der deutschen chemischen Gesellschaft* for September. Their initial difficulty was to obtain a pure cellulose, and they eventually decided to use Swedish filter paper. Even this in its purest state contains traces of impurities, which were a source of trouble. These having been removed as far as possible, about a kilogram of the filter paper was cut up, placed in a copper-retort, and subjected to distillation for about two hours. Of the distillate, consisting of tarry products and substances soluble in water, only the latter were considered. The aqueous solution was subjected to a process of fractional distillation, and by this means a series of substances were isolated. Of these, the most important are formaldehyde, furfurol, maltol, oxymethyl-furfurol, and γ-valerolacton. The occurrence of formaldehyde is rather interesting, and it had previously been shown by Pasqualis to be present in the smoke from wood fires. Furfurol was present in considerable quantity, and it is already well known as a product of the dry distillation of wood, while γ-valerolacton is known to be present in wood vinegar. The authors content themselves with the isolation and identification of these substances, and do not venture further on any theoretical speculations.

LITERARY NOTES.

YET another magazine has been added to the already considerable number of periodicals which exist for the purpose of instructing parents, guardians, and teachers in the proper care of their charges. The month of October has witnessed the appearance of *The Child*, a monthly journal devoted to child welfare, ably edited by T. N. Kelynack, M.D., and published by Messrs. John Bale, Sons, and Danielsson, Limited. This most interesting magazine differs slightly in several respects from similar publications, and therefore it "will occupy a position different from any other periodical devoted to child problems. It will not interfere in any way with the excellent journals already dealing with child study, school hygiene, and other special aspects of child welfare, but by affording a medium for co-ordination and co-operation it will endeavour to assist all forms of thought and action directed to the furtherance of the best interest of child life. At present numerous institutions are accomplishing useful service, but their workers remain in comparative ignorance of the results achieved by others, and know very little of methods beside their own. *The Child* will provide all such with a centre for information, co-ordinating experience likely to be of value for the general good, and providing means for the initiation and record of inquiries to elucidate particular problems connected with child life." *The Child*, therefore, aims at constituting a kind of an exchange bureau of general information with regard to all that concerns the welfare of children, and for this reason alone it should prove a welcome and useful addition to the periodical literature of the day. In order that its readers may profit to the full by the experience of others, contributions are welcomed in any language and from any part of the civilized world. This first number contains several interesting articles written by distinguished foreigners, whilst the list of English contributors includes the names of Sir Lauder Brunton, the Earl of Meath, the Bishop of Ripon, the Right Hon. John Burns, M.P., and a host of eminent men and women interested in the moral and physical well-being of the rising generation. In view of this imposing array of well-known names, it is, perhaps, hardly necessary to say that the contents of *The Child* are worthy of all praise, and should prove extremely useful to all those who have children under their care. Its numerous and excellent articles, all characterized by practical sympathy and common sense, should ensure a

speedy popularity for the new journal, and it may be predicted with safety that it will not be long in gaining a wide and appreciative circle of readers. It should be added that *The Child* is beautifully printed, and contains some excellent illustrations.

In a lecture delivered to the Glasgow Eastern Medical Society, and published in the September number of the *Glasgow Medical Journal* under the modest title of 'Notes on some old Glasgow Institutions with Medical Associations," Dr. Hugh A. McLean, has given an interesting account of the foundation and gradual development of the University of Glasgow. This great seat of learning, the second oldest in Scotland, came into being in the middle of the fifteenth century. It owes its existence to Bishop William Turnbull, who prevailed upon the reigning Pope, Nicholas V, to found a university in his diocese of Glasgow upon the same plan as the famous universities of Paris and Bologna. The charter of foundation was granted by the Pope in the year 1450, but though Nicholas approved of Glasgow as the site for a school on account of "the healthiness of its climate and the plenty of victuals and of everything necessary for the use of man," neither he nor anyone else appears to have taken sufficient interest in the new foundation to render possible the erection of proper buildings for the use of its scholars, and, according to Dr. McLean, at the beginning the classes were probably held in the crypt of the cathedral. It was not long, however, before the college authorities were enabled to take a house in the Rotten Row, on the site of which now stands the Lock Hospital; and in 1459 Lord Hamilton granted them a tenement in the High Street and four acres of land in the Dovehill, on condition that at the end of every mid-day and evening repast the regent and students should pray standing for the souls of their benefactor and his wife. After the first few years there seems to have been no lack of endowments and gifts for the growing college. Only eighteen years after its removal to the High Street it received a grant of land from Sir Thomas Arthurlie, and during the next century not only did Queen Mary make over thirteen acres of land in the Dovehill, with the Kirkroom of the Friars Preachers and the rents of various tenements, but her son, in 1577, refounded and endowed the whole institution. It was not until 1632, however, that the college was rebuilt, the new buildings being erected on the same site in the High Street. In those days, and indeed until much later, there does not seem to have been any "living out" system at Glasgow. All students lived in college, and though at the beginning this privilege cost nothing, later on the rent of college rooms varied from 4s. to 10s. a session. It has already been stated that the Scottish University had been modelled upon those of Paris and Bologna, and this similarity extended to the systems of tuition and examination, and even to the very textbooks in use amongst the students. The power of the college authorities was almost unlimited (they were actually able, upon one occasion, to conduct the trial of a student who had been charged with murder—students being almost outside civic jurisdiction, and entirely under the control of the college), and the discipline they enforced was rigorous to the extreme. For instance, no games were allowed except golf and archery; billiards, cards, acting (at one time permitted), and even bathing, being strictly forbidden, whilst such things as wearing a sword in town or "cutting the gown of another on the Lord's day" were regarded as serious offences. In 1677 an edict was published to the effect that "if any student occupying rooms be found guilty of breaking the glass windows or doing any other detriment to the house they shall be forthwith publickly whipped and extruded from the College." The diary of Josiah Chorley, an English student, an extract from which is quoted by Dr. McLean, gives ample proof that life at the University of Glasgow in the seventeenth century was not so pleasant as it is now. "The good orders of the College," writes the virtuous Mr. Chorley (who one cannot help suspecting was a bit of a prig) "were very agreeable to mine inclination; at 5 o'clock in the morning the bell rings, and every scholar is to answer to his name, which is then called over The Lord's days are strictly observed, all the scholars called to the several classes, when, after religious exercises, all attend the Primar and Regents to the church forenoon and afternoon. In the evening they are called again, and then come under examination concerning the sermons heard, and give account of what was appointed the foregoing Sabbath in some theological treatise, and then to supper and chambers." Bedtime was at the same time, 9.15 every night in the week, and must have come as a welcome relief after these solemn Sundays. It is to be feared that all Englishmen at Glasgow were not so easily pleased as Josiah Chorley. Early in the eighteenth century the English students appear to have raised a protest against the food provided at the college table, with the result that the Principal was forced to take the matter up, and persuaded Alexander Eagle, the cook, to "furnish with diet all Englishmen to diet within the College, during the continuance of this session, three times a day, with meat and drink, with such changes of diet as is mentioned in a paper apart: The said Englishmen and each of them paying to the said Alexander Eagle £3 for each three months' diet and so proportionally." One hopes that this arrangement worked well, and that the Englishmen were satisfied with the culinary efforts of Alexander Eagle. The price they paid for their privileges cannot be considered exorbitant, as, according to Dr. McLean's reckoning, it only came to 8d. a day for all three meals.

The *Old-Lore Miscellany of Orkney, Shetland, Caithness, and Sutherland* (vol. iii, part iii. Printed for the Viking Club. Published by Curtis and Beamish, Ltd., 50, Hertford Street, Coventry) must be a "feast of nectared sweets" to readers with antiquarian tastes. Indeed, others besides the members of the Viking Club, for whom it is primarily intended, might well be interested in this fascinating little magazine, which is a veritable mine of information on the history, literature, language, and folklore of the North of Scotland. The July number, amidst a great deal that is well worth reading, contains one particularly interesting article—a short account of "Ferchard, Physician to King Robert II," by the Rev. A. Mackay. Froissart mentions somewhere in his *Chronicles* that the Scottish king of that period was troubled with chronic inflammation of the eyes, and here we are afforded a glimpse of the physician who attended him, and who Mr. Mackay is disposed to look upon as "an early example of the eye specialist, whose skill and devotion were highly appreciated by the afflicted king." Who was Ferchard? Mr. Mackay admits that very little is known concerning this fourteenth-century oculist. There seems some reason to believe that he was the son of Iye, Chief of Mackay, in which case he was brother-in-law to the Wolf of Badenock, one of King Robert's sons, who doubtless procured for him his post at Court. The date of his birth and of his death are alike unknown, and no record exists to tell us where he studied medicine. Mr. Mackay suggests that he may have gone to Paris, as did so many Scottish youths during the Middle Ages. All that is definitely known about Ferchard, however, is that in the year 1379 he received from his Royal brother-in-law the lands of Melness and Hope in Sutherlandshire—a gift which was ratified by the King; and seven years later—on December 31st, 1386—Robert II himself granted the Little Isles of Strathnaver (which consist of all the islands along the north coast from Stoirhead in Assynt to Armadalehead in Farr) to "dilecto et fideli nostro Ferchardo leche, pro suo servitio nobis facto et faciendo" (our beloved and faithful Ferchard the leech, for his service done and to be done to us). The possession of these large estates enabled Ferchard to found a landed family of Mackays, and descendants of the old Court physician seem to have survived till as late as the seventeenth century. Some confusion has arisen concerning the identity of Ferchard, owing to a tradition which claims him as a member of the family of Beaton in Isla. These Beatons were hereditary leeches to the great clan of Maclean, and one of them became physician to James VI. Hence it has been frequently asserted that Ferchard and James's doctor were one and the same person—a conclusion which Mr. Mackay has successfully proved to be erroneous.

Mr. Morton Edgar announces the publication of a work entitled *Great Pyramid Passages*, based upon letters written from Egypt and Palestine in 1909. These letters, revised and extended by the late Dr. John Edgar and Mr. Morton Edgar, deal with the Great Pyramid and its symbolical significance. The work contains about fifty photographs of the interior of the Pyramid. Orders may be forwarded to Mr. Edgar, 224, West Regent Street, Glasgow.

British Medical Journal.

SATURDAY, OCTOBER 29TH, 1910.

EXPERT MEDICAL EVIDENCE.

It is not often that we refer in these columns to criminal trials, for though many of them, it is true, present considerable medical interest, it is rarely of a kind which can usefully be discussed. It is otherwise, however, in respect of the poisoning crime which has attracted so much attention during the last two or three months, and which reached its climax last week. Questions of the identification of human remains—few in number and long after death—and of the recognition of organic poisons, despite the processes of putrefaction, always have a special importance and interest of their own. Moreover, the bulk of the evidence on which the whole case turned was of a purely medical character, and was supplied by an unusually large number of witnesses. Indeed, the action was early characterized as a doctor's case, and even the Lord Chief Justice spoke of it in somewhat similar fashion.

For this reason it seemed well to take steps to secure for the information of our readers a fuller report of the medical parts of the evidence than was likely to be given in any lay newspaper; and we now lay before them in another part of this issue a careful summary from a verbatim report specially supplied of the evidence in chief of each of the medical witnesses, of their cross-examination, and of the comments on the medical evidence made in their final addresses by the counsel for the defence and prosecution respectively, and by the Lord Chief Justice himself in the course of his summing-up. It seemed useful to do this, because, apart from features of passing interest, there is much to be learnt from the case both directly and indirectly, and it will be years before its lessons are crystallized out in the form of brief statements in works on toxicology and forensic medicine.

Each reader will judge for himself, but there are few, it seems to us, who will not feel some degree of pride at the fashion in which the medical profession was represented on this occasion by the official medical witnesses for the Crown. Their evidence was given in a manner on which, in point alike of scientific exactitude, lucidity, succinctness, and absolute fairness, it would be exceedingly hard to improve. Especially admirable were the readiness and completeness with which Dr. Willcox proved able to answer the abstruse questions on the comparative chemistry of the mydriatic group put to him in cross-examination. The medical profession, therefore, to this extent has much reason to congratulate itself.

There is, however, another side to the picture—the official witnesses were not the only representatives of medicine engaged in the case. There was imported into it by the defence evidence of the kind to which the term "expert" is usually applied, and the impression it made on the court is unpleasantly visible in the speech of the prosecuting counsel and in the closing observations of the Lord Chief Justice. Since the manner in which evidence is delivered counts for a good deal, readers cannot absolutely judge how far these carefully worded comments were justified; but as many of the statements made by the witnesses in question are given in our report verbatim, they have before them some material, at any rate, for forming a judgement on the point.

The net outcome of the whole matter must be, it seems to us, an accentuation of the grave discontent long felt by the medical profession at the circumstances which render conflict of medical evidence so common in courts of law. We say "circumstances," because it is with these rather than with medical men, either individually or as a body, that the responsibility really lies.

At the same time, however, it may be suggested that the ethical considerations evoked by these conflicts have not yet received the attention they deserve. One reason, doubtless, is that they involve some very fine points, and that in any case distinctions must be drawn between different classes of medical witnesses. When two or more medical men have both seen a case in the ordinary course of their work and differ in respect of it, there is no doubt that in present circumstances each is bound to go into court and uphold his own view as frankly and strongly as absolute adherence to truth will permit. Medical men, again, who are the holders of official positions which necessitate their investigating cases and offering opinions thereon are in much the same position. In either case the evidence given may be "expert" in so far as those who give it are of high scientific attainments and special experience in the matter in question, but it is not to such evidence that the term "expert" is commonly applied. That term is reserved for quite another class of evidence—that given by persons who have no original or essential connexion with the case, but who have been called on behalf of one side or the other. They are persons presumed to possess special knowledge who have been induced to place their services at the disposition of solicitors, first to coach them in questions which may be asked by counsel in the hope of diminishing the effect of the scientific evidence on the other side, or of confusing the issue, and then to appear in court themselves and tender counter-evidence.

For medical evidence of this kind it has sometimes been suggested that there is, ethically speaking, no room whatever. On the other hand, it may be maintained that every man possessed of scientific knowledge is entitled to place it at the disposition of those who think it may be useful to their case, and that a medical man is as much morally justified in using his brains to help to upset either a prosecution or a defence as is a lawyer in defending his client. Against this it might be urged that the analogy does not hold good, and that to use medical knowledge for such purposes is to debase it. It is not being used either for the relief of suffering or in the highest of all scientific objects, the revelation of truth; for the "expert" medical witness must often be aware that though the medical evidence on the other side may be not absolutely unexceptionable in

every particular, it is in essential points absolutely correct.

Those who take this line call to mind, moreover, that a barrister, even when charged with the defence of the life of a prisoner, is fettered by custom in the extent to which he may make suggestions to the effect that black is white, and that even if this were not the case, barristers, as such, are not concerned with the establishment of eternal truth, but merely with the rights or wrongs of cases as judged by the rules of an absolutely conventional procedure. To medical men, however, such rules do not apply; their concern must be simply with scientific facts. These, of course, are open to various interpretations, and on such points doctors are proverbially apt to differ.

At the same time, those who on these grounds condemn expert medical evidence are quite prepared to admit that the tendering of it is fully justified when the motive is a feeling—such as may be assumed to have existed in this case—that, in default of intervention, injustice will be done. They argue, however, that the reasons for this belief must be very strong, and can rarely exist when, since a man's life or liberty is at stake, every person who gives evidence unfavourable to him must necessarily do so with extreme reluctance, and when the medical witnesses in particular are men of the highest ability and experience in the matters to which they are testifying and are known to conduct their work with the utmost care.

There is considerable force in these views, but, as was indicated at the beginning, the ethics of this question are of so difficult a kind that until they have been more thoroughly ventilated and discussed, no one is entitled, either openly or in his own heart, to condemn any man for taking an opposite view to his own, whatever that may be, or for acting in accordance with that view.

Meantime, we merely deplore the conditions which allow such unfortunate conflicts between medical witnesses to occur, and do so not only because they tend to interfere with the dignity of medicine, but because, more important still, the underlying conditions may at any time easily lead to a grave miscarriage of justice.

Given a highly skilled man but a bad witness on one side of a criminal action, and a witness having less knowledge and experience but clearness of head and readiness of wit on the other, a criminal might easily escape or an innocent person be convicted if the medical evidence were of importance and dealt with fine points involving the use of long terms and considerations unfamiliar either to judge or jury. The possibility of this must be obvious to those who, like ourselves, constantly have to study accounts of civil actions turning on medical evidence. It must often seem to them that the verdict would have been different from what it was if the court had better understood the medical bearings of the facts as represented by one side or the other, or if the medical evidence which failed to win the day had been given by a more convincing kind of witness.

We therefore hold, as has long been held by most medical men who have considered the subject, that it would be greatly to the advantage of the public and to the administration of justice if, in trials of a medical kind, the scientific evidence presented to the court were that not of an individual, but of a majority of medical men asked to consider the medical points at issue.

MILITARY MEDICINE.

Judging by some figures supplied in his presidential address by Sir David Bruce on October 12th, the United Services Medical Society is certainly entitled to be considered a very successful organization. It has only been in existence a little over three years, and has a membership roll of over 500. We draw attention to this fact because some of its members apparently regard the society as lacking vitality. It may be added that its meetings, reports of which habitually come under our notice, are of a successful and useful kind; the papers are carefully prepared, and are followed by good debates. Nor has an adequate element of "shop" ever seemed to us to be lacking. It is impossible that the subjects debated shall always be of a kind in which only members of this society are likely to be interested. Military medicine, including in that term surgery, hygiene, and organization of work in peace and war, is by no means a pure speciality, and if the "shop" idea were carried too far the occurrence might be common of debates which were of interest to army medical officers, but of none to naval medical officers, and vice versa.

Apart from other ways, the absence of true specificity in military medicine is happily evidenced by the very material contributions to existing knowledge of the etiology, pathology, and clinical phenomena of disease which have been made by officers belonging to the Services. Most of it has been the outcome of work performed by them in the course of their ordinary duties, and much more may be, and is, expected of them. Unless they unfortunately grow to regard themselves as specialists, there are few, if any, subjects on which they may not throw a light, while they have, as was generally indicated by Sir David Bruce, unrivalled opportunities of studying certain diseases of vast importance not only to the army and navy, but to the community at large. Typhoid fever, dysentery, and the effects of the *Spirochaeta pallida* and the gonococcus are cases in point, quite apart from malaria, phlebotomus or sandfly fever, and other diseases of the tropical medicine order.

The essential unity of military and general medicine is further evidenced by the fact that military and naval medical officers have at times contributed to the solution of problems even of general physiology For evidence of this statement we need go no further than the present issue, in which appears Lieutenant-Colonel Melville's report on his elaborate investigation of the amount of food required for mere sustenance and work respectively. It is one of the papers published in our account of the proceedings of the Section of Physiology at the July meeting. The author, it may be observed, dealt with the question mainly from the point of view of the effect of measured amounts of work and measured amounts of food, on the weight, general condition, and well-being of the individuals concerned.

Nor is it certain that even subjects which at first sight seem entirely of a "shop" character are really specific to military medicine and have no connexion with general medicine or any bearing thereon. As an example of such a "shop" question, that on the etiology of Soldier's Heart, which was debated at the opening meeting of the society this year, suggests itself. Hitherto it has been regarded purely as a soldier's question, and investigation into its etiology would seem to have been almost entirely confined to the features of life characteristic of a soldier's career;

these two circumstances may possibly be the reason why its causation still remains so much of a mystery.

It is true that in their more recent study of this subject soldiers have had the great advantage of the co-operation of a physiologist in the person of Dr. Pembrey, of Guy's Hospital; but it is possible that he, too, has regarded the matter too exclusively from the soldier's standpoint. It would be quite worth while to make endeavours to ascertain whether the condition known as "soldier's heart" is not fairly common among young civilians belonging to the same class as that from which soldiers are drawn. The alternative phrase by which the disease is known—D.A.H., or disorderly action of the heart—has been fairly familiar among ordinary medical men since the South African war, when this condition led to the sending home of so many otherwise useful soldiers; and it is possible that if their attention were directed to the matter general practitioners might be able to supply records of cases of the same character from among their own patients. Such records, if available, would in any case serve as controls, and investigation of the circumstances of the life of these civilians might possibly throw light on the extreme prevalence of D.A.H. in soldiers; it appears to be recognized as a disabling disorder chiefly in the third year of their service.

Its clinical features, as described by Colonel Simpson in the debate in question, are, in addition to increased rapidity of the pulse and a disorderly action of the heart, cold blue hands, defective muscular development, a low skin temperature, and a general increase in muscular irritability and of the knee-jerk reflex. These phenomena are accompanied by some increase in blood pressure, this averaging about 140 mm. Hg.

Various explanations of its pathogeny have been forthcoming, and to some of these allusions were made by those who took part in the debate. Dr. Pembrey, for instance, observed that soldiers worked at a disadvantage as compared with civilians, since they were heavily clothed. This caused a rise of temperature and dilatation of the cutaneous vessels, and thus threw a strain on the heart. Possibly, too, the sharp tone in which orders were given might not be without its influence, since the necessary instantaneous response caused an increase in the pulse-rate. Such an effect had been demonstrated in the case of firemen.

Surgeon-General Branfoot, on the other hand, attributed the condition to cigarette smoking, while Major W. S. Harrison quoted the opinion of Davy, who, some thirty years ago, attributed soldier's heart to compulsory position of "attention," which, he pointed out, was accompanied by an habitual semi-expansion of the chest and loss of action in the respiratory pump. Lieutenant-Colonel Deane, like several other members, thought clothing had something to do with the matter, but no longer believed that the position of "attention" was a factor, nor yet over-training; the men affected were soft, flabby persons. Personal experience was quoted by Sir David Bruce and others in evidence of the fact that a tight jacket during work raised the number of heart beats. It was also shown by Lieutenant Hayes that the number of heart beats increased after five minutes' standing at "attention," as compared with the rate while standing easy.

It cannot be said that any of these explanations or attributed causes taken separately is of a very convincing nature. The prevalence of soldier's heart does not seem to have decreased of late years, despite the fact that the old-fashioned stock has been done away with and clothing is in every way very much looser than it was at the time when the condition first attracted attention. Cigarette smoking, though it may very well be a contributing factor considering the age of recruits, is too common a circumstance to be a full explanation. The position of "attention," coupled with semi-expansion of the chest, is a circumstance in the life of all soldiers, of whom only a minor proportion acquire soldier's heart, and it is to be remembered in the same connexion that hundreds of men of the upper classes habitually hold themselves with braced-back shoulders and chests thrown forward—in fact, in a position which from the point of view in question is the same as that of a soldier at "attention." Doubtless tight jackets are an added strain during exertion, but the ordinary cyclist and sportsman gets through an immense amount of work in clothing not looser than the fatigue uniform of soldiers.

Indeed, looking at the matter from what may be called an epidemiological standpoint, it would almost seem that, though there may be many contributing factors, the true cause must be individualistic rather than communistic. This being the case, the most attractive suggestion would seem that made by Lieutenant-Colonel Melville, who was disposed to attribute soldier's heart to over-exertion of recruits before they are properly fed. He thought they should be fed up for a month before any work was given them. Given a number of growing youths who suddenly begin to take really hard exercise, perhaps, for the first time in their lives, and when, perhaps, they have been on comparatively short commons for a time, or living on ill-chosen food, it would not be peculiar that a certain number of them should damage their hearts sufficiently at the beginning of their soldier career to account for the after-onset of definite D.A.H.

With this conception, Lieutenant-Colonel Deane's observation that the men affected are generally soft, flabby specimens is in keeping; this type of man is often a heavy indulger in cigarette smoking.

THE ILLNESS OF PRINCE FRANCIS OF TECK.

It is less than a month since we published a letter from H.S.H. Prince Francis of Teck appealing for help for the Middlesex Hospital. In this and other philanthropic work His Royal Highness had always taken a keen and active interest. It is therefore no conventional form of words to say that his untimely death will be mourned by the whole nation, and especially by the suffering poor. It would be an impertinence on our part to present our condolences to the Royal Family, but we are sure we are expressing the views of the whole profession in saying that it sympathizes most heartily with Her Majesty the Queen and his other royal relatives on their bereavement. As some misconception of the facts of the case has found expression in certain quarters, we are glad to be able to publish the following authoritative statement: Prince Francis had been suffering for some time from an affection of the nose. After his return from Marienbad, where it had been recognized as an empyema of the antrum, an operation was performed, and the cavity was drained. On September 28th the Prince went to Balmoral, and soon after arriving there he suffered from occasional paroxysms of cough, with expectoration. He kept quiet, and was not at any time exposed to chill. On October 4th he was suddenly seized with severe pain in the right side. His temperature rose

to 102.5°, and a friction sound was detected at the right base. The signs of effusion gradually developed, and he was unable to travel south when the Court left on Monday, October 10th. On the following day a consultation was held, and it was decided that he probably had an empyema, for which an operation would be necessary. The pneumococcus had previously been found in the expectoration. As there were then no urgent symptoms, and as his general condition had somewhat improved, it was decided, after a further consultation, that he should travel to London on October 13th, and proceed at once to the nursing home. He did not suffer from chill on the journey. He was seen on arrival, and at the examination the physical signs which had been reported were found. On the same afternoon, Friday, October 14th, an exploratory puncture revealed the presence of a condition requiring early operation; this was performed the same evening. The fluid removed contained a pure culture of the pneumococcus. After the removal of the pressure upon the lung by the opening and draining of the pleura, examination showed that an excavation existed in the right upper lobe, and that the pneumococcal infection had spread from the lung to the pleura. The temperature fell to normal on the morning of October 15th, but there were no other signs of improvement. The temperature rose again for three days, and then for four days was normal. The toxic effects upon the nervous system were very pronounced throughout the illness, and in the early hours of Saturday, October 22nd, the heart began to fail, and the Prince passed away at 11 a.m.

THE STATE CHILDREN'S ASSOCIATION.

THE seventh report of the State Children's Association, covering the years 1907–8–9, shows a substantial advance on many points since the last was issued in 1907. The scope of the association's work is constantly widening. Founded in 1896 to advocate the interests of Poor Law children in the metropolis alone, it has of late years extended its operations to the provinces, and from only dealing with dependent children it has gone on to consider the case of young delinquents. The methods and organization of the association are by this time widely known. It continues as fortunate as ever in its officials, and in Lord Lytton and Sir Albert Spicer has secured an invaluable chairman and vice-chairman. Events have of late marched rapidly, and proposals which three years ago seemed counsels of perfection are to-day established facts. The Children Bill has become the Children Act. Before introducing it Mr. Herbert Samuel asked the assistance of the association, and suggestions were made with regard to the amendment of the Infant Life Protection Act, 1899, the necessity of legislation for the children of vagrant parents, the compulsory establishment of separate courts of justice for children and the special treatment of feeble-minded delinquent children. Almost all the amendments to the bill proposed by the association were obtained. The amending of the Infant Life Protection Act is a special case for congratulation, for "one-child cases" are no longer exempt from protection, the age limit is raised from five to seven years, and foster-parents can no longer insure their helpless little charges. The result of these changes has been sufficiently striking. Last year in one union alone the number of registered cases rose from 92 to 389, and the additional 297 were all one-child cases. Like evidence comes from other unions, and to those who can read between the lines this provision and the visits and advice of the protection visitors mean health and happiness and life itself to thousands of little children. The association has given its aid in promoting those courts of justice for children which the Children Act has made compulsory, and a special effort, attended by considerable success, was made to accelerate the working of the Probation of Offenders Act. A letter signed by Lord Lytton was addressed to all Chairmen of Petty Sessions, suggesting measures for ensuring its active working, and for enlisting a large body of voluntary help on its behalf. Lord Lytton was invited to sit on the Departmental Committee which subsequently inquired into the working of the Act, and the State Children's Association was requested to furnish suggestions. The association is urging the compulsory appointment of women visitors and lady inspectors under the Infant Life Protection Act and the Order regulating boarding-out within the union. The State Children's Association welcomes the corroboration of its principles given by the Poor Law Commission Reports, both of the Majority and the Minority, which declare decisively against the extension of barrack schools, and pronounce against the maintenance of children in workhouses—an abuse which, often as it has been exposed, is still too common. The association is combating strongly the use of workhouses as places of detention for young offenders, and its report points out that the Local Government Board has granted permission for their use for a limited period only, so that every effort must be made that this unsuitable arrangement shall not become permanent. Boarding-out continues to increase in popularity, and excellent work has been done by the association in forming fresh committees in neighbourhoods where none were formerly at work. In the report of three years ago fifty-eight unions were mentioned as having adopted the scattered-home system of dealing with children ineligible for boarding out. To-day that number has risen to nearly ninety, and to almost all of these the State Children's Association has been able to send information and suggestion, receiving letters of appreciation which show the hold obtained by the principles of the association. Provincial work grows apace, fresh boards of guardians are being drawn in, meetings are organized in increasing numbers, and leaflets, letters, and answers to all sorts of applications go out constantly from the office. Nothing so concerns a nation as the wellbeing of its children. The three years' record of the association's work justifies an appeal to the public for such support as will enable it to answer the calls to labour which reach it from many sides. The report can be obtained (price fourpence post free) from the office of the Association, 53, Victoria Street, S.W.

ECZEMA "CURES."

THOSE who have seen or read the well-known comedy, *A Pair of Spectacles*, may remember that one of the first incidents in the process of disillusionment by which Mr. Benjamin Goldfinch loses his faith in human nature consists of an interview with two rival shoemakers, each of whom stoutly maintains that the boots he has made are "best calf" and those made by his rival only "cow." Mr. Goldfinch, unable to decide which of the men is lying and strongly suspecting that both are doing so, is impelled logically to the conclusion that at least "they can't both be telling the truth." It would be well if the public could be induced to apply the same reasoning to the claims of competing manufacturers of proprietary medicines, whose statements are so mutually contradictory as to convince any one that "they can't both be telling the truth," and to leave a very strong suspicion that both are lying. The article which appears elsewhere in this issue of the JOURNAL

on nostrums for eczema, etc., gives instances of such statements as those referred to. For example, one maker says "sulpholine lotion is the only external skin remedy"; "eczema and all skin ailments are completely cured by sulpholine lotion"; and another says "the potent ingredients of a remedy, which are necessary to the successful cure of eczema, cannot be contained in the form of a liquid. Lotions, besides containing mercury, must necessarily contain some alcohol or water, which are irritants and should be rigidly avoided." (The implication that all lotions contain mercury is distinctly original.) While one asserts that "it may take a few months to effect a permanent cure"; and others make similar statements, yet another says "there is no need of an extended course of treatment in the cure of eczema. If the remedy is the right one, its effects will be apparent immediately and the progress of its work may be noted daily." In some cases sufficient discrepancy is shown by bringing one statement into juxtaposition with another made by the *same* nostrum seller, as in the instance where it is stated that "there is not one among the many healing extracts used which is not highly lauded by skin specialists the world over," and on the next page of the same pamphlet "several of the medicaments used in the ointments and blood tablets are very rare and not in use in Great Britain, and do not form any part of the British doctor's medical outfit." Serious inconsistency frequently exists between the claims put forward in a press advertisement, the function of which is to get the sufferer into correspondence, and the much more cautious statements that are made later, and which can, if necessary, be pointed to afterwards as showing that the patient was not justified in expecting prompt benefit; as an example of this in one press advertisement cited readers are assured that "just a drop or two of D.D.D., the Guaranteed cure for Eczema and all Skin Diseases, applied to the burning skin, and that torturing, endless, nerve-racking itching is at an end"; "not only does D.D.D. stop all irritation and pain immediately—we guarantee it—but the cures that invariably follow are permanent," while in a pamphlet supplied with this very article it is stated that "the person fortunate enough to become familiar with D.D.D. Prescription in the early stages of his disease may commit the error of being impatient of cure. . . . The eruption may have appeared only on a few spots of his body, but this is evidence that the disease germ is lodged in his skin. The germs will grow and multiply and spread; . . . it may take some time to dislodge the germs from his skin. . . . Is it not worth careful study, a few months' patience, to be rid of that horrible malady?" The medical profession comes in for the usual amount of abuse, as, for example, "many doctors don't appear to reconize the fact that the complaint should be treated both locally and constitutionally, therefore their treatment often proves ineffective" (the writer apparently does not "reconize" the rules of spelling); "notwithstanding the great strides that have been made in medicine for the past decade or two, skin diseases, and particularly eczema—the worst of them—are about as little understood by the medical profession to-day, as they were a generation back." "It may be that physicians have failed to give this trouble the proper amount of study because, not being a fatal disease, they have considered it unimportant enough to wilfully neglect its thorough diagnosis." "The average medical man is helpless to combat the growing trouble of skin diseases." The company making the first of these statements endeavours to prove that its methods are the same as those followed by the qualified medical practitioner: "When you consult a doctor he first inquires into the causes leading up to your trouble, then carefully examines all the symptoms and indications. Upon the results he forms his diagnosis, which in turn suggests the treatment. We do exactly the same when you return the examination form filled up." The trifling omissions of the years of study, training, and experience requisite for a medical qualification, and of personal examination of the patient, are treated as of no consequence; naturally, some illuminating diagnoses may be looked for under such circumstances, as in the case described, where, having received the answer "Eczema" to the question, "What are you suffering from?" and after "carefully studying" the answers to this and other questions, the company gravely informs the patient, "You are suffering from a severe and disagreeable form of eczema"!

ANCIENT ANATOMICAL DRAWINGS IN TIBET.

IN a most interesting note reprinted from the *Asiatic Quarterly Review*, October, 1910, Lieutenant-Colonel L. A. Waddell, C.B., I.M.S., points out that hitherto our information in regard to the medical and anatomical knowledge possessed by the ancient Hindus has been derived almost exclusively from the non-illustrated accounts in manuscripts. In the Temple of Medicine at Lhasa, Lieutenant-Colonel Waddell found a set of ancient anatomical drawings which preserve in concrete pictorial form the old-world Indian beliefs in regard to the structure and functions of the internal organs of the human body. These, which were obviously intended for teaching purposes, are essential to a right understanding of the history of ancient Indian medicine. The circumstances under which Lieutenant-Colonel Waddell visited the Temple of Medicine in 1904, and unearthed there these drawings are described and illustrated in his *Lhasa and Its Mysteries* (pp. 376–79). A set of the drawings, which he procured, is now deposited in the India Office Library along with the greater portion of the manuscripts and books collected by him in Tibet, numbering about a thousand volumes, and comprising very many rare works and many unique and hitherto unknown treatises, including several on medicine. Lieutenant-Colonel Waddell says that in Tibet at the present day, as was formerly the case in ancient Europe, the study and practice of medicine is confined to the priests. They are taught this subject in their ordinary curriculum at the monasteries, and those who desire to specialize come to the Temple of Medicine at Lhasa. This fane is dedicated to Buddha. That dispenser of spiritual medicine is represented as "The God of Medicine"—an Esculapius, whose image occupies the chief place in the temple, with the title of "Buddha, the Supreme Physician, the King Beautiful as *Baiḍurya* (beryl or lapis lazuli)." The author thinks that: This myth possibly incorporates that of the Assyrian healing (?) deity, Enu-rēstū, who is also associated with this stone. In this form he is a favourite object of worship by most modern Buddhists, and is the popular *Binzuru Sama* of the Japanese. The ancient Buddhist canon contains several treatises on the healing art which are ascribed to the Buddha himself, and these, with their commentaries, form the basis of the art as practised at the present day in Tibet. The source of this Indian Buddhist medical lore has not, says Lieutenant-Colonel Waddell, yet been clearly traced. Ancient Greek influence probably accounts for some of the analogies exhibited to the old humoral pathology, with its flow of the bile and the phlegm. In Buddha's day, in the fifth century B.C., Taxila, near the modern Rawal Pindi, in Northern India, was especially

famous as a centre of medical learning, and one of its most celebrated physicians at that time was Jīvaka. Certainly, after Alexander's invasion in 325–27 B.C., Greek influence was strong and remained so for several centuries before the scriptures of either of the two great branches of Buddhism were reduced to writing. And it was from this neighbourhood that the Sanskrit text of the Buddhist canon, from which the Tibetan version is translated, was first collated and issued. The Indian Buddhist origin of this particular set of pictures is attested by a painted row of twelve monks at its top, as founders of the art, and bearing Indian names of monks of Buddha's Order. They are divided into two groups, one of which is labelled: "Line of the ancient founders (or compounders)." This group commences with a Sākya, but he does not clearly appear to be the great *Muni* himself, as he bears the title of "the great Abbot," and is surnamed the "mighty," which is used in Tibet as the equivalent of the Indian *Iswara*, or *Siva*, an epithet not usually applied to Buddha. The second is called the "King Punya," who possibly may be the Prince Punya-bala, who was apparently a contemporary of Buddha, and in regard to whom there is a tale (avadana) in the Buddhist canon. None of the names of the others, however, are recognizable as those of known personages; nor are any of those of the second group, which is entitled "The thread of the pure ones." Among these latter the chief place is given to a monk, figured in the conventional attitude of Tsongka'pa, the founder of the yellow hat sect of lamas, and inscribed, "Sarvajna, the lord of doctrinal teaching." This latter list probably includes some Tibetan teachers. The date of introduction of these pictures into Tibet is not, according to Lieutenant-Colonel Waddell, evident. According to the current tradition of the temple, as related to him by the high-priest, the original of these drawings was brought from India in the remote past; but he could point to no positive evidence in support. This particular temple, although rebuilt and extended by the regent Sangyas Gyamts'o about 200 years ago, is said to have been founded many centuries before that. It seems to Lieutenant-Colonel Waddell possible that the pictures may have been brought to Tibet in the first half of the eighth century A.D. For it is stated in one of the best of the native histories that in the reign of K'ri-lde Tsug-rtan (A.D. 705–755), in addition to certain Buddhist scriptures which were translated by Indian monks from the Sanskrit into Tibetan, "Pichi- (or Pochi-) Chandra Sri translated books on medicine and surgery, on astrology, and such like subjects." The Lhasa edict of this king's son, found by Lieutenant-Colonel Waddell, states that his father was the first solidly to advance the civilization of his people; and medicine and the treatment of disease, he found, was one of the subjects which Tibetans specially inquired about. Lieutenant-Colonel Waddell gives a general description of the contents as a preliminary note, but he says the complete interpretation of the technical details will require the illuminating aid of a Tibetan physician. The drawing, he says, is made upon a sheet of loaded cloth, which rolls up like a scroll or map, 31 in. long and 26 in. across. The chief figures are three in number, delineated in black ink and coloured. The central figure displays the human body standing with outstretched arms, and in its interior are depicted the blood vessels and bile ducts, etc., as imagined; also the vertebrae. The two lateral illustrations are respectively those of a man and a woman, to show the internal organs, which are supposed to differ considerably in position in the woman. The central figure exhibits an interesting attempt to map out the proportions of the human figure with precision, according to a natural unit scale of measurement by finger-breadths, which are drawn all over the figure in squares. A man's stature is represented as being ninety-six fingerbreadths, and the proportions thus defined for the figure and limbs are accepted and followed as a canon by Tibetan artists. The positions allotted to the internal organs are only very roughly approximate, and have manifestly not been ascertained by dissection. The spinal column extends through twenty-five vertebrae to the level of the tip of the external ear, and the spinal cord to the vertex. The central figure bears the following inscription: "Chart for identification of the internal parts of the body: In the circle of veins of the five [regions], the head, neck, breast or chest, navel and privy parts are 24 superficial 'veins' in each. These are the ten sapless vessels [= arteries?] and the ten sap (or 'marrow') vessels twenty [in] each, twenty minutely sized ones; [and] the four divisions of the principal [vein], each of which has 25 [subdivisions] which are associated with 500 minute tributary ones. The vessels of the five orifices, the six sense [organs] and of generation, the blood tract of the excellent intellect, etc. The blood area for blood-letting under and in front of the forefinger for pulse-feeling. The vein which menaces life in plague. The running of the various skin-colours, white and red. The plan of the beating pulses in the head, neck, mouth, etc." The right-hand figure (female) is inscribed: "The method of disposition, on the right and left, of the five deep veins for ascertaining the cause [of disease], and the superficial middle vein of the mystical circle of the breast." The left-hand figures bears the legend: "Delineation of the inner vessels [showing] the mode of connexion of the eight great veins of the hidden systems." Among these curious notions of anatomy and physiology Lieutenant-Colonel Waddell finds that of the three classes of blood vessels, the first bears the name of "the sapless" (*roma*), which, he says, recalls the ancient Western conception of the artery as an "*air* duct," from which our name for it is derived, as in the dead body the arteries were always found empty, and hence were supposed to convey air, a mistake which prevailed even down to Harvey's day. He adds that a fantastic result of the notion that the relative position of the blood and bile vessels in a man are reversed in a woman is that the physician feels with his right hand the left wrist for the pulses in a male patient (as the right is supposed to have only bile ducts), whereas he feels the right wrist of a female, employing for this purpose his own left hand.

OPTICAL EDUCATION.

A CORRESPONDENT writes to draw our attention to the claims of technical optics to a better building and equipment at the hands of the London County Council. He supports this by quoting from the Report of His Majesty's Inspectors of the Board of Education for the Session 1909–10, in which they point out that the present accommodation at Clerkenwell Horological Institute is quite inadequate and unsuitable, and that the present department of technical optics in that building is overcrowded. In spite of these conditions surprisingly good work has been done, and, in the opinion of the inspectors, the department is well worthy of the effective aid of a new and suitable building. The Education Department of the London County Council seems to be in favour of this, and the matter has received the support of an important meeting of opticians, over which Mr. James Aitchison presided. There can be no doubt of the national importance of the wellbeing of this trade. Some years ago England was in the forefront as maker of micro-

scopes, telescopes, lighthouse reflectors, and similar instruments, but during the last thirty or forty years a great part of the work has gone to Germany. How far this is due to want of skilled workmen or how far to the question of wages we cannot decide, but that we have to lament the transfer of a large bulk of the manufacturing opticians' trade is beyond question, and we feel sure that improved technical teaching offers the best prospect of bringing it back to these shores.

IRREGULAR MEDICAL PRACTITIONERS.

THERE is one phase of the Crippen trial which should be particularly noted both by the State authorities and the general public. The crime could hardly have been accomplished had it not been for the fact that Crippen, thanks to his American degree, was enabled to procure poisons in any quantity he desired. Despite the fact that he had been associated with the notorious and now defunct Drouet Institute, and that he was an active employee in a quasi-medical institution which figures in *Truth's* Cautionary List, he was accepted as a regular practitioner of medicine even by the chemists with whom he dealt. Indeed the fact that he called himself consultant physician to an advertising American firm, named Munyons, which supplies pilules of which an analysis and full report appear in *Secret Remedies*, seems to have been one reason why they were prepared to deal with him. They did not always even require him to sign their poison book, and supplied him on his demand with a quantity of a highly dangerous drug, such as hyoscine, larger than they kept in stock. But for this circumstance the chemists can hardly be be held to blame, since it has become the custom in this country to accept foreigners who dub themselves medical men at their own valuation. It is quite time that this free trade in medical practice came to an end, and for both the public and the authorities to adopt and maintain the legal interpretation of the term "medical practitioner." This is a person who in virtue of an approved curriculum at an approved institution has obtained admission to the *Register* kept by the General Medical Council. There are certain countries which take just as much care over the education of their medical practitioners as does Great Britain, and exercise over their work the same supervision as is exercised by the General Medical Council in this country; if men known to be the holders of medical degrees from such countries and to be of good repute therein, arrived in Great Britain the public would have no cause to concern itself particularly, and medical men for their part would merely shrug their shoulders. But America is not one of these countries. Attempts have been made of late years to improve medical education in the States, but how small is the degree of success yet reached is amply demonstrated by the recent report of the Carnegie trustees. A large proportion of the degrees obtainable in America are not only absolutely worthless from the point of view of this country, but are frankly admitted to be so by Americans themselves. Nor is the fact admitted only by individuals; it is recognized also by the American State authorities, none of whom permit to practise any one and every one who arrives from some other State claiming to be the possessor of a degree granted therein. Unless the degree which he claims to possess is absolutely known to be a thoroughly good one, and to have been obtained by the claimant after a proper period of education, attempts to practise by him are punishable by fine and sometimes by imprisonment. It is perfectly absurd, therefore, that when America takes so many precautions to protect itself against its own people, we in this country should accept any individual who arrives claiming to be an American graduate of medicine as worthy not only of confidence, but as one whose services should rather be sought than avoided. It may be added that any such individual may be put down with certainty as practically more or less a quack when he adopts advertising methods or poses as a specialist in one department of medicine or another. This is the case because among reputable medical men in the United States medical ethics and etiquette are not materially different from what they are in this country, and no medical man who respects himself and who should be respected is likely to do in a foreign country and among foreign professional men what he dare not for his professional reputation's sake do amongst his own countrymen. These men are dangerous to the community and their existence and the harm they do adds one more to the many reasons which call for an early amendment of the medical Acts. A more practical form of Medical Act would save the public both in pocket and in person and prevent the honourable title of doctor of medicine being so frequently degraded.

WANDSWORTH SCHOOL CLINIC.

WE are informed that the Wandsworth Division is to hold a meeting on December 2nd, in the evening, at the Town Hall, to which all members of the local medical profession, members of school care committees, and heads of departments of all schools in the area, have been invited. The object of the meeting is to discuss any points of difficulty, so that the clinic may open under the best possible conditions on January 1st, 1911.

SIR JOHN BYERS, M.D., has been elected, for the third year in succession, President of the Belfast Natural History and Philosophical Society.

THE King has conferred the honour of knighthood on Dr. James Kingston Fowler and Mr. Alfred Pearce Gould, under whose care the late Prince Francis of Teck was after his return to London. His Majesty invested them with the insignia of the Knight Commandership of the Victorian Order on October 26th. Sir James Kingston Fowler is Senior Physician, and Sir Alfred Pearce Gould Senior Surgeon, to the Middlesex Hospital.

THERE are vacancies for two officers in the 2nd London Sanitary Company (Territorial Force). The Administrative Medical Officer will be glad to hear from any medical man engaged in sanitary work who is willing to join. There are also vacancies in the Field Ambulance, with head quarters at Greenwich, and several regiments in the 2nd London Division are still without their complement of medical officers. Particulars can be obtained from the Administrative Medical Officer, or the Staff Officer, Craig's Court House, Whitehall, S.W.

A CONFERENCE of some leading members of the medical profession in London and clergy is to be held at the Chapter House, St. Paul's Cathedral, this day (Friday, October 28th) to discuss the respective positions of the medical adviser and the minister of religion in cases of sickness. The conference is, we understand, the outcome of a book recently published under the editorship of Mr. Geoffrey Rhodes advocating the close co-operation of the clergy and doctors to cope with the modern tendency for irregular healing of various kinds.

England and Wales.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

LONDON.

BOARD OF EDUCATION AND MEDICAL INSPECTION.

THE receipt of the letter from the Board of Education on the subject of the organization of medical inspection in London (summarized in the BRITISH MEDICAL JOURNAL of October 22nd, page 1272) was reported on October 26th to the Education Committee of the London County Council by the Children's Care Subcommittee. After recalling the decision of the Education Committee on July 20th, 1910, to provide for the inspection of entrants and of children between the ages of 8 and 9, and 12 and 13, and of other selected children (BRITISH MEDICAL JOURNAL, July 30th, 1910, page 293), a scheme not yet approved by the Council itself, the subcommittee recommended that a reply to the letter of the Board of Education be sent by the Clerk.

The proposed letter explained that the cause of the delay in replying complained of by the Board was the long midsummer recess, and set forth the result of the Education Committee's deliberations mentioned above. The communication then proceeded—taking for granted, it will be noticed, the future consent of the Council—on the following terms: "A copy of this paper (containing the Education Committee's proposals) was forwarded to the Board in the usual course. In view of the long recess it was found impossible to bring this scheme to the Council before October 25th, and the Council *on November 1st approved without modification* the scheme which had been brought informally under the notice of the Board. The Council gathers from the Board's letter that the Board do not suggest that the number of officers proposed by the scheme referred to will not be sufficient, under normal circumstances, to carry out the requirements of the Education (Administrative Provisions) Act for 1907, and of the Code for 1909–10, as well as to devote attention to the other phases of medical inspection which are not required by the Act and by the Code, but on which the Council lays great stress. It is suggested, however, by the Board's letter that owing to the date on which the last of the staff will be appointed, it will not be possible to carry out before July 31st, 1911, the requirements of the Code for the current educational year. In this connexion I am to state that the Council has devoted great attention to this aspect of the question, and I am to assure the Board that the requirements of the Code as at present understood will be carried out fully within the current educational year."

The draft letter was approved.

OPTICIANS AND SPECTACLES FOR SCHOOL CHILDREN.

With reference to the objections of the Worshipful Company of Spectacle Makers and the British Optical Association to the statement in "Health Hints to Parents," recently issued by the London County Council, to the effect that spectacles should only be worn when prescribed by a doctor, and should never be obtained from an optician or "eyesight specialist" unless prescribed by a doctor (BRITISH MEDICAL JOURNAL, August 13th, 1910, p. 399), the Children's Care Subcommittee reported to the Education Committee on October 26th that it was convinced it was not possible, without risk of error, to test the vision of school children without the administration of a drug which opticians were forbidden by the terms of their diploma to use. The Council, therefore, should advise parents to consult a medical man in respect of any defect of vision in their children. At the same time the subcommittee thought the pamphlet should be so modified as to remove any suggestion which might be taken as a reflection upon opticians. The following wording was suggested:

> The children may require spectacles, but these should be worn only when prescribed by a doctor. The children's eyes should never be tested by opticians or eyesight specialists, as hese do not use the necessary drugs.

The suggested alteration was approved.

MANCHESTER.

HENSHAW'S BLIND ASYLUM.

THE increasing demand for fresh accommodation for the blind has compelled the board of management of Henshaw's Blind Asylum at Old Trafford to make additional provision, and on October 18th the Mayor of Salford opened the Hayesleigh Home and Workshops for Blind Men and Women. The Hayesleigh estate was presented to the board about three years ago by Mr. C. H. Scott, and for the last two years the house has been used as a secondary school for blind boys. The board, however, recently decided to concentrate all the educational work at the main institution and to transfer the older inmates to Hayesleigh. The house has been reconstructed and workshop accommodation has been added, where the able-bodied men will be employed in making baskets, mats, and brushes. Henshaw's Asylum is one of the oldest philanthropic institutions in Manchester, and the Mayor of Salford thought that the board was acting on right lines in teaching the blind to work. He also spoke highly of the educational work for blind children at the asylum. The institution was opened in 1837, and there are now abont 200 resident pupils—100 in the elementary and 100 in the technical department, while at the new Hayesleigh home there are 42 adults who are either engaged in work or enjoying a rest in their old age. After the ceremony of opening, the company present were conducted over the new home and workshops. Many of the children at the asylum are sent and partly maintained by various educational authorities, Salford sending 16, while Manchester, which has its own day school for the blind, sends 9 children.

ST. MARY'S HOSPITAL.

At a meeting of the Board of Management of the St. Mary's Hospitals, Manchester, held last week, a letter was read from Mr. Edward Hopkinson, who recalled the fact that his father was the predecessor of Sir F. Forbes Adam as chairman of the Southern Hospital before the amalgamation with St. Mary's, and that during his chairmanship he collected funds for instituting a number of beds or cots. Mr. Hopkinson then went on to offer a donation of £2,000 to the funds of the charity for the maintenance of two adjacent beds in the gynaecological wards of the new hospital in York Place, to be named in perpetuity the "John Hopkinson" and the "Alice Hopkinson" beds, in memory of his father and mother. The gift, however, is made conditional on the new hospital being opened for the reception of patients within six months' time. The Dean of Manchester draws public attention to this last condition, and to the fact that he also had been enabled some time ago to offer £500 to the funds, but on the same condition, that the hospital should be opened within six months of November 1st. The coincidence of two offers with the same condition attached is regarded by the dean as showing how wide is the desire of many of the citizens of Manchester that the new hospital, which is completed but remains unused through want of funds, should not remain closed indefinitely.

HOPE HOSPITAL.

The Salford guardians are again in difficulties about Hope Hospital. On the retirement of the late medical superintendent, owing to ill-health, the guardians advertised for a successor, and it was made one of the conditions that applicants must have had "previous administrative experience in a Poor Law or general hospital." A medical man resident in Salford, who would doubtlessly bave admirably filled the post, was appointed by the guardians out of something like fifty applicants, but when the appointment was laid before the Local Government Board for the necessary confirmation, that Board pointed out that the necessary condition of having had previous administrative experience was not fulfilled by the candidate appointed, though there was not the least doubt of his general ability as a medical practitioner; the Board therefore refused to confirm the appointment. The guardians

sent a deputation to the Local Government Board to urge it to give its assent, but after reconsideration the Board maintained its refusal. The guardians thereupon passed a resolution reiterating their opinion that their choice was the best possible under the circumstances, and several guardians, especially the chairman of the Hope Hospital Committee, were inclined to defy the Local Government Board. In the end, however, the Local Government Board insisted on its determination not to assent to the appointment, and required the guardians to make a fresh appointment. They have at last seen the futility of defying the Local Government Board, and have decided to advertise again. The new superintendent ought to have commenced his duties at the hospital on October 1st, but owing to the delay caused by the dispute with the Local Government Board the post has not yet been filled up and the hospital has for several weeks been without any resident medical superintendent, and the whole of the work has been in the hands of locumtenents.

Hulme Dispensary.

The annual meeting of the subscribers to the Hulme Dispensary was held in the Lord Mayor's parlour, Manchester, on October 21st, under the chairmanship of the Lord Mayor. The committee state in their report that the number of patients treated during the year was 4,446. According to the rules, persons whose wages are higher than a certain amount, which varies according to their responsibilities, are not accepted as patients. The report says that this rule has been strictly carried out and the committee is satisfied that only proper people were receiving the benefits of the charity. Last year 2,500 persons who were unable to pay the sum of 1s. which is charged for three weeks' attendance were treated gratuitously. The number of accident cases has risen from 85 in 1905 to 315 last year, and this is said to be partly due to the steady work of the house-surgeon, who not only takes charge of all the accident cases but pays frequent visits at the patients' homes. On the advice of the auditors the securities had been revalued, and owing to depreciation of the stock the accounts showed a total loss of £383, but it was explained that this would only affect the charity if it had to realize any of its securities, as had been the case in some past years. There was, however, no need for any such realization this year owing to the success of the special appeal for funds that had been made. Lord Egerton of Tatton drew special attention to the very small cost of administration considering the large amount of good work done. Mr. C. T. Needham mentioned that there were 62,000 people living in Hulme, which worked out at 130 per acre, against only 34 per acre for the whole city. This obvious overcrowding meant a high death-rate and a high disease rate, and he cordially supported a resolution, which was proposed by Mr. Colefax, M.P., thanking the public for the response made to the special appeal of the treasurer and asking for continued support for the dispensary.

Royal Infirmary Old Residents' Club.

In October, 1908, shortly before the demolition of the old Manchester Infirmary, a gathering of old residents was held to take farewell of the old hospital. At the dinner then held a Residents' Club was proposed. On Saturday, October 15th, of this year the first dinner of this club was held at the Midland Hotel, Manchester, 157 old residents being present. Mr. Southam, who was probably known personally to more old residents than any other member present, was president, and Dr. W. O. Jones, Mr. A. Boutflower, Dr. Johnson, and Dr. Pilkington acted as vice-presidents. Excellent speeches were made by Dr. Pilkington, Dr. Arnold-Jones, and the President, but acting on the experience gained at the last dinner, when the speeches suffered from the hum of talking between friends who had not seen each other for years, the committee arranged but little speech-making. There was a general feeling in favour of another dinner being held in three years. It is hoped that members who were unable to be there this year will then make a great effort to be present, for nothing helps so much to the success of the meeting as the presence of all co-temporary old residents.

WEST YORKSHIRE.

Bradford Medical Societies.

Last week saw the inauguration of the work for the session of the Bradford medical societies. On Tuesday, October 18th, the Medico-Chirurgical Society met in the Board Room of the Royal Infirmary. An excellent microscopical demonstration was given by Dr. Eurich, and specimens were shown by Drs. Mitchell, Jason Wood, and Martin. The new President, Dr. W. Mitchell, gave his presidential address, with the title of "*X*-Ray Diagnosis." This was most interesting and instructive, and was illustrated by numerous lantern pictures.

On Wednesday, October 19th, a meeting of the honorary staffs of the Bradford hospitals was held in the Board Room of the Royal Infirmary. Dr. Campbell was elected Chairman for the ensuing year. The chief subject of discussion was the new infirmary scheme for Bradford. It is held by many people in Bradford that an effort should be made to accommodate all the Bradford hospitals on the new site purchased by the Royal Infirmary Committee. A resolution giving effect to this feeling, but reserving the autonomy of each institution, was adopted after considerable discussion. But it is impossible to ignore the fact that the staffs of the two institutions which it is desired to draw into the scheme—the Children's Hospital and the Royal Eye and Ear Hospital—were either antagonistic or without enthusiasm in the matter. Consequently, the adoption of the resolution could only be considered one of academic interest, and as voicing the feelings of the majority present.

The opening meeting of the Bradford Division of the British Medical Association for the session took place at the Great Northern Hotel, Bradford, on Thursday, October 20th. Previous to the meeting, Dr. Bronner, Honorary Secretary of the Yorkshire Branch, very genially and generously entertained to dinner the retiring Chairman (Dr. Metcalfe), the Chairman (Dr. Bampton), and the Honorary Secretaries (Drs. Dunlop and Willson), officers of the Bradford Division.

Dr. Bampton gave a most interesting address, with the rather unusual title of Medicated Squab Pie. This was an intermixture of personal experiences of, and reflections on, the course of his medical career. Dr. Metcalfe read a report on the two recent Representative Meetings. He pointed out some of the salient features of the meetings, and said that he had no doubt whatever that the Association, in spite of some gloomy prognostications to the contrary, had a great and brilliant future before it. He had been elected as a member of the Central Organization Committee—a compliment he took not only as a personal matter, but as a recognition of the great work done by the Bradford Division for the Association. It was decided to hold a Divisional supper, to be followed by a smoking concert, on Wednesday, November 23rd. This will, no doubt, be as great a success as its predecessors.

Bradford Anthrax Investigation Board.

A meeting of this board was held at the offices of the Bradford Chamber of Commerce on October 20th. Dr. Eurich reported that 6 cases of anthrax had occurred since the last meeting in May. All were of the external variety, and all had recovered. During the same period 151 samples of wool and hair had been tested for anthrax. Of these, 124 were bloodstained, and anthrax germs were cultivated from 9 of the bloodstained samples.

WALES.

Memorial Fountain to a Medical Man.

An interesting function took place at Taibach, Port Talbot, on October 15th, when Sir Arthur Pendarvis Vivian, K.C.B., unveiled a drinking fountain, erected

by the inhabitants of the district, as a memorial to Dr. J. H. Davies, J.P., for the services he has rendered to the district. Sir Arthur P. Vivian, in unveiling the fountain, expressed his pleasure at being able to do such honour to the esteemed doctor. Dr. Davies had laboured amongst them for a great number of years, and had won the esteem of the rich and poor alike, especially the latter, to whom he had been a great and true friend. Dr. Davies, in reply, expressed his thanks to Sir Arthur for performing the ceremony, and also to the inhabitants and committee for erecting such a memorial to himself after fifty-eight years' labour amongst them. He asked Mr. D. Wellington, the Vice-Chairman of the Margam District Council, to accept the fountain on behalf of the council. The fountain is the work of Mr. Goscombe John, R.A., and was subscribed for by the inhabitants of the district, the cost being 350 guineas.

MAESTEG INFANT MORTALITY.

At a meeting of the Maesteg Urban District Council held on October 18th, the Medical Officer of Health, Dr. Kirkby, in his report for the month of September, said there were 28 deaths, half the number being of infants under 1 year of age. He was still of opinion that the Notification of Births Act, 1907, should be adopted as a means of reducing the infant mortality, and at the same time a lady health visitor should be appointed. Dr. Kirkby added that he was of opinion that the services of a lady health visitor might be secured for about 25s. per week. The recommendation of the medical officer, however, was not adopted.

Scotland.

[FROM OUR SPECIAL CORRESPONDENTS.]

EDINBURGH.

Edinburgh and Leith Medical Practitioners' Association.

THE annual dinner of the Edinburgh and Leith General Practitioners' Association was held at 129, Princes Street, Edinburgh, on Thursday, October 20th, at 7.30 p.m.; the President, Dr. T. R. Ronaldson, was in the chair and Dr. Crawford Renton of Glasgow was the guest of the evening. There were 85 gentlemen present. Dr. Crawford Renton proposed the toast of "The Association," and expressed the desire that Glasgow had a similar, living, and energetic association representing her general practitioners. In reply the President reviewed the work of the association during the past year, specially referring to what had been done in dealing with the evils of club practice, and in regard to establishing a nursing home in Edinburgh, where the rates for maintenance and nursing shall be from a guinea upwards, one-third of the beds available being at the minimum rate. He also referred to the work the association had in view for the coming year, the first being the necessity for the municipal authorities having an adequate control of the milk supply and the sources of the milk supply of the city, which now it certainly had not. The second question was that now being raised by the Local Government Board, in urging that the municipal authorities should forthwith provide an adequate hospital for a possible outbreak of small pox. That hospital might either be one for the city situate in a safely remote locality, or might be a conjoint hospital for Edinburgh, Leith, and the county.

Research Defence Society.

A meeting of the members and associates of the Edinburgh Branch of the Research Defence Society and their friends was held on Friday evening, October 21st, when Mr. Stephen Paget gave a lecture on "Pasteur's Life and Work." The Lord President of the Court of Session, the Right Hon. Baron Dunedin of Stenton, P.C., K.C.V.O., LL.D., etc., presided, and there was a large attendance. On the platform were Sir John Batty Tuke, Sir A. R. Simpson, the President of the Royal College of Physicians (Dr. W. Allan Jamieson), Professor Schäfer, Dr. Geo. A. Gibson, and the two Secretaries. Apologies were intimated from the Duke of Montrose (President of the Branch), the Earl of Cromer (President of the Society), Sir William Turner (one of the Vice-Presidents), the President of the Royal College of Surgeons, Sir A. Conan Doyle, the Rev. A. Wallace Williamson, D.D. (one of the Vice-Presidents), Sir J. Halliday Croom, Professor Alexis Thomson, the Rev. John Kelman, D.D., the Rev. Gavin L. Pagan, B.D., and many others.

Dr. John D. Comrie, one of the secretaries read the annual report, and said that the council suggested the following office-bearers of the branch for the ensuing year: President, the Duke of Montrose, K.T.; Vice-Presidents, Lord Dunedin, Sir William Turner, K.C.B., F.R.S., Sir John Batty Tuke, M.D., LL.D., and the Rev. A. Wallace Williamson, D.D.; Honorary Treasurer, Professor Harvey Littlejohn; Honorary Secretaries, John D. Comrie, M.B, F.R.C.P.E., and R. W. Johnstone, M.D., F.R.C.S.E.

Professor Schäfer moved the adoption of the report, and Mr. J. M. Cotterill, Vice-President of the Royal College of Surgeons, in seconding, spoke very strongly of the deep debt of gratitude that operating surgeons lay under to the research of scientific men, who had enabled them to give themselves the great privilege not only of saving many valuable lives, but also mitigating an enormous amount of human suffering and misery by the exact methods that had been taught by these researches.

Lord Dunedin, in introducing the lecturer, said the Research Defence Society was founded for the purpose of making known the facts relating to experiments on animals, the regulations under which these experiments were conducted, the precautions which were taken to render them, as far as possible, painless, and the benefits both to mankind and to the lower animals which had resulted from them. No doubt, the objects of the society might be in a certain sense described as polemical objects, but they were certainly not aggressively polemical. They were only polemical in defence, and, speaking for himself and for many others who in different departments of life were interested in the general progress of humanity and of their nation, but had no claim to rank themselves among scientists, he should not have assumed, as he had, a position in the society unless he had thought that it was high time such a society was formed. If it had not been formed, there was a real danger that scientific research, with all its beneficent results, might be practically hindered by opposition, no doubt in many cases based on well-intentioned ideas, but based, as he believed, a great deal upon ignorance and upon an inability to make deductions which ought to be drawn. A wish had been expressed to him that at that, the first meeting of the branch, what he might call the polemical side of the branch's operations should not be specially insisted upon, and accordingly the subject of the lecture that night was not any thesis upon the necessity of certain experiments, but was to be a history of actual facts connected with the life and work of the great man who had contributed to research in science, and who, though in a rough way to those who were ignorant, was known as the man who first with success combated one of the most dreadful diseases, which up to his time was practically thought incurable. The lecturer was the son of Sir James Paget—a name revered in the history of English surgery—and was himself a practising surgeon and interested in the movement for the protection of research, and able to give his testimony to its value. Let them remember that what was claimed for research was not that it was an advance in scientific knowledge, although that was in itself something, but that it was the actual, and, indeed, if rightly understood, the only, means for the bettering of the practical strife against death, disease, and pain. It was because research was in the end intended to destroy pain that it was liable occasionally, when it was impossible to avoid it, even to inflict pain.

Mr. Stephen Paget then gave his lecture, illustrated by many lantern slides. He spoke of Pasteur's first great discovery in chemistry in 1848, his study of ferments beginning in 1854, his discovery of the causes

of fermentation of milk in 1856 which led Lister to think of the causes of wound infection and thereby to the methods of antiseptic surgery; Pasteur's work on the diseases of silkworms which were destroying the silk industry of Paris, his studies on the prevention of diseases in man and beast, his studies on fowl cholera and preventive inoculation, his investigation of anthrax and its vaccine and the brilliant results of this, his work on the methods of antiseptic surgery in the Paris hospitals in 1875, his investigation as to the true cause of puerperal fever in 1878, and finally the discovery of the preventive treatment of rabies, which had led to a mortality almost infinitesimal.

Dr. Geo. A. Gibson moved a vote of thanks to the lecturer, and the President of the Royal College of Physicians to Lord Dunedin for presiding.

Epidemic of "Infantile Paralysis" in Edinburgh.

During the last three months a large number of cases of acute anterior poliomyelitis have occurred in the Edinburgh district. So many cases, indeed, have come under notice since the beginning of August that one is justified in considering the condition to have been present in epidemic form. As is not uncommon when such a condition prevails, many of the cases have presented atypical and unusual symptoms. Thus, while the ordinary age-period for the onset of the disease—the second and third year—has been that when many cases occurred the infection has also attacked children of 7, 8, and 9 years. Cerebral symptoms have been comparatively common. These have varied in degree from restlessness and hypersensitiveness to prolonged coma, paralysis of cranial nerves, and, in at least one case, death during the acute onset of symptoms.

Edinburgh Water Supply.

Considerable excitement and anxiety have been created among the citizens of Edinburgh by the premature and unauthorized publication in the daily press of an incomplete report prepared for the use of the Water Trust by Dr. James Ritchie, superintendent, and Dr. J. P. McGowan, bacteriologist to the Laboratory of the Royal College of Physicians, prior to its being in the hands of the trustees. At a committee meeting of the Water Trust on Thursday, October 20th, views were expressed that the publication of a report regarding an investigation which was as yet incomplete was reprehensible in the extreme and detrimental to the interests of the trust, besides being misleading to the public. Thereafter the Lord Provost moved that it be remitted to the Convener, Bailie Cullen, and Councillor Rusk, along with the clerk and law agent, with powers to make the fullest investigation with the view of ascertaining from whom or from what source the public press had obtained a copy of Dr. Ritchie's report or information therefrom, and to report. Meanwhile, to allay public anxiety, Dr. Ritchie has written and published a letter, in which he says:

> I think it right to state that there are, in my opinion, no grounds for anxiety. The water delivered from the filters both at Fairmilehead and Alnwickhill is of excellent quality, and will compare most favourably with the water used in any other large city in the kingdom. The only point disclosed by my analyses is that the Fairmilehead water (which constitutes only one-tenth of the total amount daily supplied by the trust) does not come up to the high standards laid down as to the quality of an urban supply. There is reason to believe that the defects noted can be remedied by such steps as the trustees have now authorized to be taken, and the working of the filters during the period of further probation will be controlled by constant bacteriological examination.

In the report referred to there is an account of the results of chemical examination, bacteriological investigation, the presence of excretal organisms — notably the *Bacillus coli* — probably coming from wild animals or farm stock. It is added that coming from such sources they are probably generally harmless to man. It is only when they are derived from human sources that they are a source of danger to consumers of a water.

After discussing the relative merits of sand and mechanical filtration, the following general conclusions are tabulated:

1. During the period over which the inquiry extended the raw Talla water was, from the chemical standpoint, relatively pure, and the ingredients on which the filters chiefly act were not present in excessive amount.
2. Filtration had practically no effect on the saline elements of the water or on the free ammonia.
3. The filtration process diminished the organic constituents by about a quarter.
4. The filtration caused a slight rise in the total hardness of the water. It diminished the temporary hardness, and markedly increased the permanent hardness.
5. From the bacteriological standpoint, the raw water during the period of observation contained a very considerable number of bacteria developing at blood heat. It was also constantly under suspicion of containing excretal bacteria in relatively small amounts of the water, namely, 1 to 10 c.cm.
6. The filtration process did not bring the filtered water up to the standard degree of purity usually required in potable waters, namely, that the water shall not contain more than 100 bacteria per cubic centimetre.
7. The filtration process did not satisfactorily remove organisms of the excretal type.
8. There is evidence that after being washed the filters did not at once attain their maximum capacity for acting on the water, and that at the end of the twenty-four-hour period they again were acting unsatisfactorily.
9. The alumina during the period of our observations was of good quality, and did not contain arsenic.
10. The filtered water appeared generally to be free from alumina. When evidence of its presence existed in the filtrate the amount was, from the public health standpoint, negligible. So far as our observations go, most of the alumina passed out in the effluent when the filters were washed.

The Medical Officer of Health (Dr. Maxwell Williamson) gives the assurance that there is no evidence whatever that the health of the city has in any way been affected by the presence of bacteria in the water. The health of the city was never so good in any previous September as in the last.

GLASGOW.

Medical Inspection of Scholars.

The Glasgow local committee of the Educational Institute of Scotland has prepared a report on the medical inspection of school children, which falls to be considered by the meeting of the association next month. This report draws attention to the variation in the methods of conducting the medical examination in the various centres in Glasgow, Govan, Edinburgh, and the counties of Stirling, Dumbarton, Renfrew, and Lanark. Considerable variation is found even in one county. The committee recognizes that in the early stages of the working of the new system criticism must be to an extent withheld, but even at this stage it feels that it is justified in submitting the following suggestions as guiding principles:

(1) The ordinary work of the school should not be interfered with by excessive demands on the teachers' time; (2) the educational work of the teachers should not be brought into disrepute by requirements which might lead to conflict with the parents; (3) duties requiring medical skill, however simple, should not be imposed upon the teachers. Teachers might reasonably be asked to forward to the local education authority or medical officer the names and addresses of the children to be examined; to distribute, by means of the children, notices to the parents informing them of the date of the medical inspection; and to supply such particulars respecting each child to be examined as were contained in the ordinary school records—for example, name, address, age, standard, and regularity of attendance. Teachers should not be required to fill up and sign notices to parents respecting the holding of the inspection or the necessity of future advice and treatment; measure and weigh children; test children's eyesight, examine their teeth, or examine them for vaccination marks; enter on school cards details respecting state of clothing and footgear, cleanliness, nutrition, speech, mental condition, work outside school hours, parents' occupation, number of rooms and of persons in house occupied by the child; dress and undress children as the inspection proceeded; fill in doctors' observations on the cards as the inspection proceeded; enter all the details given on the cards in duplicate in a schedule or register; fill and sign notices to parents advising them that treatment was necessary, or inquiring as to action taken.

The procedure varies in different parts, even as regards the medical details, and it is recognized that some standard method of conducting the inspection should be laid down for guidance.

Ireland.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

CONOLLY NORMAN MEMORIAL.

AN interesting and impressive ceremony took place on St. Luke's Day, October 18th, on the occasion of the unveiling of the memorial to the late Dr. Conolly Norman at St. Patrick's Cathedral. The Lord Lieutenant, who performed the ceremony, made an appropriate and sympathetic speech, to which the Dean of St. Patrick's replied on behalf of the Cathedral Chapter. The memorial, which is placed in the north choir aisle, consists of a medallion portrait in high relief in bronze of Dr. Conolly Norman, and is set in a foliated niche of freestone. The bas-relief is the work of Mr. J. M. Carré, and is surmounted by Dr. Norman's crest, also in bronze. A bronze panel beneath bears the following inscription:

Conolly Norman, M.D. (Hon. Causa), Univ. Dub.; Vice-President, Royal College of Physicians of Ireland; for twenty years Medical Superintendent of the Richmond Asylum, Dublin; President of the Medico-Psychological Association, 1894-5; a true friend, an able physician, an ardent student of science, a man of varied culture and pronounced individuality. Born 12 March, 1853; died 23 Feb., 1908.

In the afternoon of the same day an oil portrait of Dr. Conolly Norman, painted by Miss Harrison, was formally handed over to the President and Fellows of the Royal College of Physicians in Ireland by a deputation of the subscribers to the Memorial Fund, and was accepted on behalf of the college by the newly elected President, Dr. Hawtrey Benson.

NEW DISPENSARY AT JERVIS STREET HOSPITAL.

There was a large attendance of the general public at the opening ceremony of the new extern department at Jervis Street Hospital on October 17th. His Excellency the Lord Lieutenant, who presided, referred in his speech to the excellent work which the hospital has performed during the last two hundred years, and called special attention to the importance of the new department owing to the fact that, from the position of the hospital in the city, large numbers of accident cases are brought for treatment at all hours of the day and night. The new building, which has been erected at a cost of £4,000, contains accident reception rooms, consulting rooms, a pharmacy, and an emergency operation theatre, and has been fitted throughout on the most modern principles. A portion of the necessary funds for its erection was supplied by the trustees of the Pembroke Irish Charities Fund, and the public are now requested to contribute the remainder.

TYPHUS FEVER AT LETTERMULLEN.

At a recent meeting of the Board of Guardians of the Oughterard district, the following resolution was proposed and adopted:

Now that the fever epidemic at Lettermullen is practically at an end, we desire to place on record our deep sense of the obligation under which Sir Acheson MacCullagh has placed us owing to the able and energetic measures adopted by him to prevent the spread of the fever. We are convinced that the steps taken by him were mainly instrumental in confining the outbreak within the reasonable compass in which it originated. We also desire to express our thanks to Dr. Francis for his able and successful treatment of the afflicted patients under his care.

WE have received from Messrs. A. and M. Zimmermann, 3, Lloyd's Avenue, E.C., copies of three booklets relating to preparations for which they are agents. One contains chemical and pharmacological accounts of all the special preparations of Kalle and Co., of Biebrich-on-Rhine, and the others summaries of the literature relating respectively to Nastin, a product introduced by Professor Deycke for the treatment of leprosy, and to Scarlet R., a dye which many investigators have found useful in promoting the growth of epithelium over the surface of large open wounds.

Correspondence.

STATE SICKNESS AND INVALIDITY INSURANCE.

SIR,—In his letter in the JOURNAL of October 22nd Dr. Fothergill presents a series of questions covering most of the bearings of State Sickness and Invalidity Insurance upon the medical profession, more especially in regard to medical attendance under such a scheme. It is to be hoped these questions will be carefully weighed by every member of the profession. The answers given to some of them are of the most vital moment to our future welfare, and although it is unlikely there will be unanimity of response on all points, it is at least desirable there should be a fairly general agreement on main principles.

The first principle involved is that of *professional freedom*. I am very strongly of opinion that it would be one of the gravest calamities which could befall our profession if we sold our great birthright of freedom for the mess of pottage—contract practice—at so much per head per annum, on the scale which seems to be contemplated by the powers that be. Under contract practice, at whatever fee per head, we bind ourselves to attend for a fixed fee for a long period; we must go whenever sent for, however trivial, however untimely; and however unsatisfactory our patient's behaviour and reception of our treatment, we must continue to attend; we have no authority to ensure that our treatment is given even a fair chance of success, and we have to bear the brunt of its failure. It is difficult, if not impossible, to preserve the proper feeling between doctor and patient under such conditions, and to obtain the best results of treatment. Not only, therefore, would our professional status be lowered, but our scope for scientific work would be greatly contracted, and our practical usefulness be much impaired by the general adoption of contract practice. Moreover, the verdict of experience both in this country and abroad is to condemn contract practice as wholly unsatisfactory.

The principle of professional freedom implies, of course, equal freedom for our patients in choice of medical attendant, and of relinquishing his services if not satisfied with them.

The second principle involved is that of *payment according to work done*. Under contract practice we accept limited pay for a practically unlimited amount of work. Experience shows that services which are to be had for practically nothing are apt to be valued accordingly, and tend inevitably to degenerate to the same level. It is not "the least medical attendance that patients will be satisfied with," but "the most efficient attendance" that should be provided, and this cannot be done unless a reasonable rate of remuneration be allowed to the medical attendants. It is quite a false idea of professional honour to suppose that there is anything "unprofessional" in our opposing, and if necessary combining to oppose, the exploitation of our services either by societies or Governments.

Self-preservation may be "the first law of Nature," but it seems to have been a law which has been largely disregarded by the medical profession, and we are now paying—and are likely, it seems, to pay—somewhat dearly for our neglect of this law. If the medical profession is capable of doing great and noble work for the people of this country—and who doubts that it is?—it is surely worth preserving in the interests of the community as well as of its members. The debt which civilization owes to the medical profession is incalculable, and it is an ever-increasing one; and whilst we are bestowing benefits on our countrymen, we are often enough removing or impairing our own means of livelihood.

The alternatives to contract practice are payment by salary and payment according to work done.

There is, of course, no objection to payment by salary, provided that the salary be an adequate one, the hours of duty reasonable, and a sufficient allowance be made for necessary study, holidays, recreation, and provision for illness, old age, etc. It is, however, of the very nature of medical practice that the hours cannot be limited, there is bound to be a good deal of "overtime," and it is also doubtful if the people of this country would at present submit to treatment by specially salaried doctors—unless, of course, *all* doctors were salaried, and the patients could

have their choice of medical attendant. It would then fall rather hard on the doctor who happened to be a favourite; he would have most of the work to do, and yet only be receiving the same pay as the man who, because he was unpopular, had very little to do. If you try to adjust this, you are introducing the principle of payment for work done. Under present conditions, I fear, therefore, payment by salary is not practicable.

After all, payment for work done—that is, for services actually rendered—is the fairest and most equitable arrangement all round; and I confess I cannot understand or agree with those of my colleagues who maintain "that the Government will never agree to payment according to work done."* The arguments used by these objectors are: (1) That it would be too costly; (2) that it is liable to abuse.

Moreover, the success of the National Deposit Friendly Society proves that payment according to work done need not be too costly.

As regards (1), if a reasonable minimum fee per visit be charged it cannot be too costly, otherwise it means that the work has to be done by the medical profession for less than a reasonable fee, which, again, means exploiting the profession. It would be a very short-sighted policy of any Government to introduce a scheme which depended on exploitation of the medical profession, and any such scheme would be condemned to early failure, and would be unsatisfactory both to patients and doctors.

As regards (2) abuse, no account is taken of the abuse likely to result to the doctor from unnecessary calls, etc., under the contract system, but great stress is laid upon the possibility of medical men making unnecessary visits when they know their fee is assured. It has been repeatedly shown in these columns how readily effective safeguards against such abuse can be devised; but it is surely far better that a few possibly unnecessary visits should be paid than that insufficient attendance should be given, and whilst it would be a grave error to press too hardly upon the individual attendant's judgement of what constituted "necessary" visits, it would not be difficult for properly-appointed medical supervisors to detect and deal with flagrant instances of abuse (for example, by disallowing fees, imposing fines, etc.). The incidence of sickness is so uncertain, the extent of attendance required in any particular instance so unpredictable, and the details of such attendance are so individual and personal, that any system of prepayment for medical services, under sickness insurance, *en masse* or by contract, is bound to be unsatisfactory.

I earnestly urge my fellow practitioners to hold fast to these two main principles—namely, (1) retaining professional freedom, (2) insisting on payment according to work done and not by contract.—I am, etc.,

Bowdon, Oct. 23rd. P. R. Cooper.

Sir,—Your readers will, no doubt, be interested in the details so well and laboriously got together by Dr. Fothergill on this important subject, and I cordially agree that it is a subject that must be faced by all branches of the medical profession. For my own part, as I do not agree with the main proposition laid down, it is unnecessary for me to follow him through all the complexities that arise, if the proposed State sickness and invalidity insurance is to include medical attendance.

With regard to his first proposal, I answer emphatically, No! In the interests of the community, or the medical profession, I think it would be far better that there should be no scheme of medical attendance. It will be quite sufficient if the State institutes a compulsory insurance system whereby all the insured are guaranteed an adequate sick allowance during illness or disablement, it being left to them to provide for themselves the medical attendance they require. The public medical service now being organized by the British Medical Association should, no doubt, if properly worked, attract a good many, but to force all the large class it is proposed to enroll in the State insurance to accept medical attendance proffered by the State would mean ruin to many of the existing medical practitioners of the country. The vested interests of the present practitioners have a right to be considered. There are many now living on the fees paid by members of the class to be insured as private patients; there are more who gain a livelihood chiefly through payments made by the same class in various forms of contract practice, all of which cannot be stigmatized as hopelessly bad. What must be the effect of enforcing contributions from all of these for any form of State medical attendance? It practically deprives them of the choice of a doctor altogether. Though quite satisfied with their present doctors they would necessarily shrink from a double medical burden. They would protest that their means forbade their continuance with their present doctors if they are obliged by the State to contract for special medical services.

The many difficulties that must arise in endeavouring to include medical attendance in State invalidity insurance make me sanguine that the Government will not attempt such an experiment. The friendly societies of the country will certainly oppose it, and I earnestly hope the British Medical Association will not attempt to press it. There are doubtless many defects in our present medical practice—far be it from me to deny them—but they are the growth of many years, and cannot be done away with by a brand new invalidity insurance enactment. Their reform must come by degrees, and can no doubt be largely assisted by our Association, working within its own province, and properly considering the wishes and the welfare of its members. But to more or less ruin the practices of many of its members would hardly promote union. I specially warn those who favour the principle of "payment for work done" against any scheme of State medical attendance. No doubt we all favour that principle speaking broadly, but I allude to the proposed payment of club patients by means of special fees for each item of work, which, in my opinion, must always be inadequate, instead of by capitation fees. If the State took in hand medical attendance in their insurance scheme, the capitation system would be nearly certainly chosen, if it were not a system or whole, or part time salaried officers; as there is literally no reliable experience of any other.

To assume that a State provision of medical attendance for the insured would be best for the community is, in my opinion, a wholly unwarrantable assumption. The State scheme, after all, might be no better than our present. I am certainly not a convert to the modern doctrine that the State can always do better for individuals than they can do for themselves. Objections might easily be raised to compulsory insurance at all; but at least let it be as limited as possible; let the members of the community have as much free choice as possible in the ordering of their lives.

To Dr. Fothergill's second proposal, should the scheme include the Poor Law medical service, I again submit an emphatic negative. The Government scheme is, presumedly, to encourage thrift, to render the working classes more independent. The Poor Law deals with the helpless and should be kept for that class. They can contribute practically nothing towards their subsistence, and are wholly dependent on the State. If this invalidity scheme is a success, it is possible that pauperism may be largely diminished, but it is more than likely that under any scheme there will still remain a sufficient number of the helpless to engage the attention of a separate Poor Law medical service.—I am, etc.,

London, N.E., Oct. 22nd. Major Greenwood.

Sir,—Dr. Fothergill's very pertinent questions in the Journal of October 22nd should be the means of eliciting the opinions of a large number of medical men on the points raised. The whole question is one of vital importance to the medical profession, and its consideration may be involved before any definite conclusions are arrived at. It is imperative that the subject should be tackled at once, so that we can with unanimity urge our views on the Chancellor of the Exchequer. Otherwise we shall be ignored, and probably irreparable injury done to our future position. The perpetuation of the club or the present Poor Law methods of payment would be disastrous to the medical profession. Let us make a supreme effort to obtain definite payment for work done. The objection urged is that the means of regulating this form of payment for medical attendance is impossible. This is a fallacy. The difficulty is not an insuperable one, and the advantages to the medical profession, to the State, and to the indi-

* They forget that the Government at present pays for the vaccination service according to "work done."

vidual are enormous. They cannot be given in a short letter of this character, but many of them are obvious to every one with a wide knowledge of general practice. It would probably be the most suitable method of eliciting the opinions of the medical profession to have a referendum of all its members. This could no doubt be best done by the British Medical Association.

The management of a scheme would be best vested in a board comprised of the various interests involved. The Poor Law Medical Service would, I think, have to be included. There would have to be both central and local management boards, the doctor receiving the confirmation of his appointment from the central board, his directions from the local board, and making his professional arrangements directly with the patient. Payment from the contributories would have to be made through the local board. The benefits would have to be regulated by some form of wage limit.

The whole scheme would have to be compulsory and contributory.—I am, etc.,

Bradford, Oct. 23rd. JAMES METCALFE.

SIR,—While agreeing with Dr. Collinson that in the event of a fight a war chest may be useful and even necessary, I should like to insist on the possibility which the occasion offers of achieving our purpose without a fight. I have confidence that the "sweet reasonableness" of our request for justice and fair dealing may appeal to the minds of our statesmen. We can show very clearly, I think, that cheap medicine and sweating have in the past worked badly for the health of the nation, and it is on this ground that we can appeal to the Government to do really well for the nation by relieving the medical profession of the intolerable conditions under which they do their work. Poor pay means overwork, and overwork means bad work; bad work deteriorates the man who does it, but what of the community he treats? This is the lever to move the Government towards equitable dealing. Let us urge the abolition of contract work for the sake of the nation's health and the progress of medicine, and I think it will not be necessary to insist on our determination to achieve better conditions of practice for our own sakes.—I am, etc.,

Bristol, Oct. 22nd. HARRY GREY.

SIR,—I desire to call attention to the important discussion on this question at the Bath and Bristol Branch, reported in the SUPPLEMENT of October 15th.

Dr. T. M. Carter moved:

> That any scheme of State sickness insurance should be based on the principle of payment for work done, as in private practice.

The resolution, which was ably supported by Dr. Grey and seconded by Dr. Michell Clarke, was carried by 61 to 1. It was thus abundantly clear that the meeting had no confidence in the tender mercies of the Government in the matter of adequate remuneration for medical services.

Dr. Parker's *non possumus* attitude did not commend itself to the meeting. He said:

> It had been suggested that they should oppose the whole idea of the Government with regard to sickness and invalidity insurance. It did not matter what their individual views might be of the right or wrong of the matter, this insurance was bound to come, and, as far as such objections were concerned, they were in the position of a fly protesting against the oncoming steam-roller.

Surely the profession will not adopt the craven attitude suggested by this simile and calmly submit to any vote-catching proposal the Government may bring forward at their expense.

It is time the Association recognized the hand of the Radical politician in the speeches and writings of its more noisy and militant members. The underlying principle with such is State control all round, which, as far as the individual practitioner is concerned, is synonymous with slavery. With any Tom, Dick, or Harry which the exigencies of party may place at the head of the department under which he will have to work, the lot of the medical practitioner will not be a happy one. He will have to work for a miserable pay, with the added horror and worry of official inspection by the emissaries of a heavily-salaried chief.

Let the profession take warning in time, or we shall one day find our independence gone, crushed by the relentless heel of the State, and Dr. Parker's "fly" will verily be under the steam-roller.—I am, etc,

October 21st. FOREWARNED.

THE HEARING AFTER OTECTOMY.

SIR,—I see that Mr. Yearsley goes for me again in the JOURNAL of October 15th with renewed vigour. He quotes from the *Transactions of the Sixth Otological Congress* (1899):

> The large majority of cases will yield to irrigation,

but leaves an important preceding sentence unquoted:

> So long as disease is confined to these spaces and *has not affected the bone*, I believe it may be reached, and consequently influenced, by injections through a large perforation.

I am of the same opinion still. I think moderate men will not approve of attacks that depend upon this sort of thing.

The first paragraph of the paper referred to contains the real cause of offence.

> Some aural surgeons recommend extensive perforations of the bone. In uncomplicated cases I believe these operations to be generally unnecessary.

So far from its being true that I then denounced all operative treatment, this very paper gave several cases in which some minor operations had been performed. I have never said or written that the incus was less often diseased than the other ossicles. I wrote that the incus was comparatively rarely diseased, and that is true of all the ossicles in my experience. In p. 88 of *Infected Ears* I write:

> The ossicles are less often affected than the walls of the middle ear, and I think there is a practical disadvantage in applying the old term to operations that include the removal of disease wherever it may be possible to remove it, and which may or may not entail the removal of an ossicle.

It is still more misleading to suggest that I advise otectomy in every case. I reserve it for cases in which there is reason to believe the bone is affected, and it has proved a most safe and satisfactory aid to cure in such cases.

Having dealt with definite statements, I will not trouble to notice here mere abuse and innuendo. I have, on the other hand, received encouraging support from some of the first men in the profession of surgery, and, indeed, of aural surgery. One eminent surgeon wrote:

> I quite agree. Too much heroic drilling and gouging.

Another, whose opinions are always received with respect, wrote:

> I think you have proved your point most clearly as to what you can do.

I have also a letter from a recognized authority on otology expressing agreement with my views as to the value of intrameatal surgery.

In conclusion, let me say that I regard the watch as a fair practical test of the hearing, and I see Dr. Hunter Tod does so too.

One word more. Mr. Yearsley says I have written of "my operation of otectomy." Let me reply in the words I once wrote to Dr. Hill:

> I cannot find this expression in anything I have by me, but if I have written of "my" operation no objection can reasonably be taken, as my methods were built up without any assistance from any one else, and in spite of the otological teaching of the moment.

—I am, etc.,

London, W., Oct. 17th. FAULDER WHITE.

SIR,—I have read with pleasure the letter of Dr. William Hill, which appeared in the BRITISH MEDICAL JOURNAL of October 8th, on the present-day treatment of suppurative diseases of the ear. It is now recognized by all aural surgeons that, although there are many dangerous cases which can only be successfully treated by either "a complete radical or a mitigated radical operation," still there

are a large number of chronic cases of tympanic disease which can be permanently benefited by minor permeatal procedures.

I cannot agree with Dr. Faulder White that a wave of surgical prejudice in favour of mastoid operations has cast a dark shadow over intrameatal surgery. The details of the special operative procedure practised by him under the title of "otectomy" appear to me to be very similar to the minor operations which are done every day by aural surgeons with considerable success. He tells us in his letter that permeatal surgery has been deplorably neglected in our country, and I am confident he is right if he refers to the practice of thirty years ago, when in every town and village in the land patients could be found suffering from permanent deafness and disorders of the ear which had never received any kind of treatment. Parents then ignorantly neglected the aural troubles of their children and regarded "a running of the ear" as a constitutional error, which ought not to be rashly checked; but fortunately all these old myths are rapidly passing away by the progress of education and the growing influence of our science, and to-day minor operations are performed in the early stages of suppurative otitis with the best results. Dr. Faulder White does not consider it surprising that 70 per cent. of his cases obtain improved hearing power after undergoing the operation of otectomy, and this is certainly a favourable result; at the same time, it does not indicate greater success than that which follows all intrameatal operations, provided the destructive changes have not extended to the nervous structures of the internal ear.

In conclusion, I desire to congratulate Dr. Faulder White on his successful management of the interesting case of a lady who had suffered from an infected ear from early life, and upon whom he performed the operation of otectomy. Dr. Faulder White's silence on this occasion was, indeed, golden. She had consulted many specialists before, and they had acted in the same judicious manner, but the result of the successful operation evidently came upon her as a very agreeable surprise, which, no doubt, added greatly to the pleasure she experienced from the unexpected increase of her hearing power.—I am, etc.,

Portsmouth, Oct. 18th. J. Ward Cousins.

FEMINISM.

Sir,—I have read with much interest the instructive editorial entitled "A Candid Friend on Feminism," in the British Medical Journal for October 15th, and I agree with you that "the problem is one of the greatest social importance," and that "the time seems ripe for a full discussion of the subject."

From Dr. Kenealy's remarks it would seem possible easily to modify important natural functions by external influences, and, as a matter of fact, a condition to be daily taking place. This is, however, doubted by many observers, and, in order to support any such argument as Dr. Kenealy's, definite statistics regarding allied conditions are necessary. Broad generalizations based on impressions are notoriously misleading and often worse than useless.

As regards the ability of women to suckle their children, I have collected the following facts which may be of some interest in this connexion. My statistics[1] (somewhat limited I must admit) refer to 415 cases collected at the Royal Hospital for Sick Children Dispensary. They were unselected cases taken in order as they appeared on the days on which I attend—93.8 per cent. of the mothers had milk at the beginning; 89.2 per cent. had sufficient milk for the first month; 62.2 per cent. suckled their children for at least six months; and 30.5 per cent. for twelve months or longer.

Of the 102 children weaned before six months, 48 were taken off the breast during the first month; 25 during the second month; 19 during the third month; 5 during the fourth month; and 5 during the fifth month.

Of those weaned during the first month the causes were: Disappearance of the milk, 17 cases; abscess of the breast, 7 cases; puerperal fever, 4 cases; illness of the mother, 4 cases; child not having strength to suck, 3 cases; child being sent out to nurse, 3 cases; menstruation, 1 case; mother going out to work, 2 cases; unknown, 7 cases.

Weaning during the second month was occasioned by illness of the mother in 8 cases; milk disappearing in 4 cases; illness of the child in 3 cases; abscess of the breast in 2 cases; mother going out to work in 2 cases; milk not being considered good in 1 case; child refusing to suck in 1 case; causes unknown in 4 cases.

In the third month no cause was given in 5 cases; in 7 cases the milk disappeared; in 2 cases the milk was not considered good by the physician; in 2 cases the child was ill and resident in a hospital; in 2 cases the child did not seem to be thriving; and in 1 case the child would not drink.

During the fourth month illness of the child, abscess of the breast, disappearance of the milk, and the child refusing to drink were each responsible for 1 case of weaning, while in 1 case no cause was assigned.

During the fifth month the child refusing to drink, the milk not being considered good, the milk disappearing, mother menstruating, and unknown cause were each responsible for 1 case.

Perhaps the most interesting feature in the above figures, so far as the present issue is concerned, is the large proportion of children weaned on account of accidental causes—namely, illness of the child or mother. In only 30 per cent. of the cases weaned before the sixth month was the cause due to insufficiency of milk. In only 14 per cent. of all the cases was there at any period before the sixth month an inadequate supply of milk for the infant's needs.

Now, as already mentioned, the above figures were obtained at the Dispensary of the Sick Children's Hospital. The women who attend this dispensary are of the poor or lower working classes, the classes from which we usually consider degenerates are recruited. There is no question of them being over-educated or indulging excessively in sport and games, but neither can one say that their upbringing, education, and hygienic surroundings are conducive to the production of healthy mothers. And so far as "feminism" is concerned they are as innocent of it as their mothers, or perhaps even their grandmothers, were before them. Nevertheless, in their lives they are subjected to many evil influences, and yet there is no evidence that they are degenerating and becoming unfit to perform the duties of mothers.

The above figures of mine are high as compared with those of Keller,[2] who found that 78.6 per cent. of the mothers of the poor and artisan classes in Vienna suckled their children at the outset, but that only 29 per cent. continued breast-feeding for at least six months, and would seem to indicate that in Glasgow, at any rate, the industries followed by these classes were not of a "de-sexing" nature.

The great disparity between this class and the upper classes, as estimated by Holt and Vincent, would seem to suggest that there is not universal degeneracy of the race, and that the incapacity of the latter, that is, the upper classes, may be ascribed, as Dr. Kenealy insists, to "feminism." Neither Holt nor Vincent in their text-books give definite statistics, but merely express general opinions. Holt[3] says that only one out of every four women of the upper classes is fit to suckle her child, and that only for a limited period; and that it is phenomenal to come across a mother who can perform this maternal duty for six months. Vincent[4] is even more general in his reference to the subject.

In comparing figures it is important that we should confine our conclusions to strictly comparable classes. It is thus questionable how far we can arrive at correct opinions by comparing different nations, even different towns, where not only local conditions, but the degree and extent to which "feminism" has penetrated, vary so enormously.—I am, etc.,

Glasgow, Oct. 18th. Leonard Findlay.

[1] *Glasgow Medical Journal*, October 1910, p. 246.

[2] *Wien. klin. Woch.*, xxii, 18. 1909.

[3] Holt. *Diseases of Infancy and Childhood*. London, 1909, p. 168.

[4] Vincent. *The Nutrition of the Infant*. London, 1910, p. 32.

Under the will of the late Captain Henry Boyles Murray, of South Kensington, the West London Hospital benefits to the extent of £1,050, while St. George's, King's College, St. Thomas's, and St. Mary's Hospitals each receive £1,000. Sums of £500 each are left to the London Fever Hospitals, the Great Ormond Street Hospital for Sick Children, and the Victoria Hospital for Sick Children.

Medico-Legal.

THE CRIPPEN TRIAL.

Special Report of the Medical Evidence.

In view of the medico-legal importance of the trial before the Lord Chief Justice of Hawley Harvey Crippen, which ended in a verdict of wilful murder of his wife, known as Belle Elmore, we thought it well to get a verbatim report of the medical evidence. Of this evidence the following is a summary. The main medical issues were whether the remains found were those of a woman, whether that woman was Belle Elmore, how long they had been buried, whether part of the remains were the wall of the sub-umbilical abdomen, whether this part bore a scar, and whether the cause of death was hyoscine poisoning.

The net outcome was that all the questions raised by these issues were held to have been answered in the affirmative. The trial commenced on Tuesday, October 18th, and ended on Saturday, October 22nd. The crime dated back to the end of January, and no suspicion was aroused until the early part of July.

Crippen, it may be noted, gave in evidence and before arrest an account of his career. He was born at Coldwater, Michigan, in 1862, his father's name being Augustus Crippen. His mother was now dead, and his father lived in California. He was educated first in Coldwater, and finished his education at the Homoeopathic College at Cleveland, where he took the degree of M.D. He came over to England in 1883, and attended various hospitals to see operations. He returned to the States, and was assistant for three or four months to a Dr. Porter, of Detroit. After that he went to New York, returning to Detroit, where he remained for three years as assistant to the same doctor. He practised as an eye and ear specialist for some years. Before going there he was married to a woman named Charlotte Bell, in New York. He had only one child by his first wife who died in 1890 or 1891. They were living at the time at Salt Lake City, where he was practising as an eye and ear specialist. Crippen then went to New York as assistant to a Dr. Geoffrey, of Brooklyn, and about 1893, when with Dr. Geoffrey, he first met Belle Elmore, who was being attended by Dr. Geoffrey. Her name at that time was Cora Turner. She was only about 17 years of age, and Crippen was about 30. She was at that time living under the protection of a stove manufacturer named Lincoln, of New York. After getting married they went to St. Louis, where Crippen practised as consulting physician to an optician. After staying there for about a year they returned to New York, where he took the position of consulting physician to the Munyon's Company. He was in New York for only a few months when the company transferred him to Philadelphia. He was there about a year, and was then transferred to the firm's place at Toronto, where afterwards he managed their business. Later on, about 1900, he settled in England in charge of Munyon's Remedy Company. He was also connected at one time with the Drouet Institute, and more recently with a quack aurist business.

In his account of his relations with his wife, Crippen said she suffered from bilious attacks, and he had given her homoeopathic medicines for that.

Mr. A. J. Pepper.

Crown Evidence in Chief.

Mr. Pepper, Consulting Surgeon to St. Mary's Hospital, said that on July 14th he went with Chief Inspector Dew to 39, Hilldrop Crescent. He met Dr. Marshall there. In the cellar he found that part of the floor had been pulled up, and in a hole in the ground he saw what appeared to be animal remains. He looked at the soil to see what it was composed of and found it was partly loam and partly clay. Mixed with these there was lime. The remains were removed to the mortuary in a shell. They included, besides some tufts of hair, a large piece of flesh composed of skin, fat, and muscle from the thigh and lower part of the buttock of a human being, and another small piece. The head was missing, and there was no bone nor part of a bone, but, except the organs of generation, all the internal organs were found.

Counsel: On the 15th July did you find a piece of skin with some fat attached to it?—Yes.

Will you describe what that was?—Well, there was fat attached to all the pieces of skin.

But did you find a piece measuring 11 in. by 9 in.?—Yes.

The Lord Chief Justice: This is in addition to the two pieces that he spoke of before?

Counsel: Yes, my lord. (To the witness) Where did that come from?—That came from the upper part of the abdomen and lower part of the chest.

Did you find another piece of skin 7 in. by 6 in.?—I did.

Where did that come from?—The lower part, the front portion of the abdomen.

Was there anything upon that which attracted your attention?—There was a mark.

Did you afterwards examine that piece with particularity?—I did.

What length of time did you spend in examining it?—Several hours altogether.

In your opinion, as the result of that examination, what was that mark?—It was the mark of a scar.

What was the length of the scar?—A little over 4 in.

That would have been visible upon the piece of skin?—Yes.

When that piece was in position on the human body where would it be?—In my opinion it was in the middle line in front; it may have been a little to the left. It began just above the pubes, and extended for 4 in. or a little over.

Did you come to the end of the scar in that piece, or did the piece end before you came to the end of the scar?—The whole scar was complete.

And was there a piece of flesh beyond it?—Yes.

Was that a new scar or an old scar?—It was quite an old scar.

Was there any trace of any genitals at all?—No.

Or any certain anatomical indication of sex?—No.

Was there any hair on that piece of flesh?—Yes.

In your opinion what hair?—Pubic hair.

The viscera, the witness said, must have been extracted from the body by a person skilled in removing viscera.

The Lord Chief Justice: That is skilled in dissection, I suppose?—Dissection.

Counsel: Of human beings?—Well, I would not like to go so far as that, but certainly in evisceration of animals.

Skilled in the dissection of human beings, or other animals, or both?—Yes, there was no cut or tear in any part, except where it was necessary for the removal.

And it was removed all in one piece?—In one piece.

The Lord Chief Justice: What do you mean by "in one piece"?—The organs I have described.

That is what I want to know; I think it is an important matter. Do you mean all the organs or the viscera only?—All the organs were connected together; and the diaphragm or the septum between the chest and abdomen had been necessarily cut round of course.

That is what you refer to as to what is necessary to get them out; but, apart from that, all these organs had been taken out by some hand so that they were still attached one to another?—Yes, and uninjured.

Counsel: And in your opinion would that require skill?—Certainly.

Now, there were no organs of generation there at all?—No.

May some of them have been removed in life?—Oh, yes; not unlikely.

Would that scar which you saw be in that position if an operation was performed for the purpose of removing some of the organs of generation?—Yes.

The Lord Chief Justice: The scar is in a place which corresponds with an operation for the removal of the ovaries?—Yes.

Counsel: Now, did you form an opinion as to the length of time that those remains had been buried?—I did; from four to eight months. In forming that opinion he had taken into consideration the place where they were buried, the surrounding materials, and the depth.

And the time after death which in your opinion they had been buried?—They were buried very shortly after death.

You took all that into consideration in forming your opinion as to the length of time?—Certainly.

In your opinion is it possible that those remains could have been buried there before the 21st September, 1905?—No.

Continuing, the witness said he put some of the things he had described into jars. Some of the contents could not be produced; they had been used by Dr. Willcox for analysis.

The witness had measured the hair found in the Hinde curler. The longest hairs were 8 in. and the shortest 2½ in.; of course, there are intermediate lengths. In his opinion, being of that length and bleached partly, the hair would be a woman's. The dark brown, the part not in the curler, and light brown in the curler. The dark brown was the hair of the root. The roots were present. The hair that was placed by him in the third jar was now decidedly darker than when he saw it. He was referring particularly to that part in the curler.

Asked to look at the portion of the hair he found in the handkerchief, the witness said he thought it was slightly darker now. This was in both cases partly due to the drying, and being more compressed together. Possibly also the bleaching effect had gone off somewhat. On August 5th he examined some hair at St. Mary's Hospital: the greater part was 2 to 3 in. long; the longest 6 in. The colour was dark brown shading off to light brown. Asked if that had altered since he examined it on August 6th, Mr. Pepper said he thought it was somewhat darker; they were all a little darker, but the first one was very markedly so. On August 8th he examined two loose hairs at the mortuary. They were lying loose on a part of the abdominal wall; one was 5½ in. and the other 8 in. They were of the same colour as the other hair he had seen—dark brown. It was a woman's hair. Mr. Pepper added: I do not know whether you wish me to refer to the hair that I examined that I believed to be pubic hair. Those hairs were twenty in number; some were free from the surface, others were still fixed.

The Lord Chief Justice: By "free" you mean not attached to the skin?—Quite so, my lord; dark brown in colour, corresponding with the undyed hair. They were from ½ in. to 1 in.

in length; they showed the roots at one end, and the other end tapered off. In all the other specimens the hair, of course, had been cut—roots at one end, cut at the other. This showed roots at one end, and the other end tapered.

You mean in a natural condition as regards cutting?—Yes, or rather the absence of cutting.

Proceeding, the witness said he found the stomach and the kidneys, and the heart and the liver and the pancreas all healthy as far as one could tell. There was no sign of disease nor anything to account for death. The spleen was very soft as one would expect from decomposition. The intestines were healthy. The lungs were more advanced in decomposition than the other organs, except the spleen, but there was no consolidation and no sign of there having been pleurisy—no marks on the surface. From the remains he examined the person in life was of stout figure. The kidneys were in an exceedingly good state of preservation. On September 9th he was present at St. Mary's Hospital when a piece of the skin, bearing the mark which he said was an old scar, was removed for microscopical examination. He was also present when that piece was microscopically examined on September 13th by Dr. Spilsbury. He examined it himself. Having examined it in that way, he was confirmed in his previous opinion that it was a scar.

Cross-examination.

Taking human remains by themselves, it was quite impossible to tell sex except upon anatomical grounds. To remove these organs in the way they were removed would require a practised hand and eye. If a person had once learnt how to do it he could do it. It was not a minute dissection; it was a particular kind of work. He formed an opinion as to the length of burial on July 15th. When he went to 39, Hilldrop Crescent on July 14th Mr. Dew was there, but he did not remember that he was told that the woman was said to have disappeared about February 15th of that year. He agreed that he would have heard that very soon after if not on the 14th. He agreed that it was quite beyond the reach of science to determine with accuracy the period of death from the progress of putrefaction, and that "two different bodies buried in the same soil and under apparently similar conditions frequently presented such differences as to baffle all attempts at generalization."

Asked by the Lord Chief Justice if lime or clay retarded putrefaction, the witness said there were two kinds of decomposition taking place in dead bodies—one where it was freely exposed to the air and warmth, and the other where it was damp and largely excluded from the air. That was what happened in this case. Lime and clay would retard the common form of putrefaction, but the presence of damp clay would favour the change which happened in this case. It was not putrefaction in the ordinary sense. It was a peculiar change; the tissues became converted into a kind of soap, the technical name of which was adipocere.

In reply to Counsel, Mr. Pepper agreed that buried in clay, adipocere would be created more quickly, and ordinary putrefaction would be retarded. Asked if taking a person of Mrs. Crippen's age and build he could form any idea as to what the normal weight of the kidney of a woman like that would be, Mr. Pepper said it would vary from 3 to 4 ounces. It might be as much as 4½ ounces. He agreed that that would be the normal weight for a woman of that height and build. He thought the weight of the kidney in this case was 2¾ ounces. It was subject to considerable variation. That would be due largely to removal of moisture from the kidney—desiccation. There was some change in the kidney; he did not want to use the word "putrefaction."

In reply to further questions, the witness said he did not accept the suggestion that there was ordinary putrefaction; there was very decided formation of adipocere. Before he formed an opinion that the mark on the skin was a scar he had already heard that Mrs. Crippen had had an operation. He could not pledge himself as to the date. It might have been close on a week after he had first seen the remains.

Asked if it was not the case that running at right angles to the navel there are what are called tendinous intersections, the witness answered in the affirmative. Those white fibres were at right angles to the vertical axis of the body.

Asked if there were ordinary tendinous intersections at right angles in that sense on a level with the bottom of the breast bone, Mr. Pepper replied in the affirmative.

Asked if there were also as a rule tendinous intersections at right angles between those two points—the navel and the bottom of the breast bone—the witness said, "Yes, three of them."

Asked if there were also tendinous intersections as a rule a little below the navel, the witness said there might be, but that varied very much. It was not so common as the other.

In reply to Counsel, the witness said he believed a navel must originally have been upon that piece of skin. It was possible, of course, that it was not. If his opinion was right, it must have been removed from that piece of skin.

In reply to further questions, the witness said there was no trace whatever there of the tendinous intersection running at right angles to the navel. He added that that scar was not so long as the wound of the operation would have been.

Asked to point out what it was agreed to call the horseshoe depression and the place where the navel was, at the top of the horseshoe depression, the witness did so. He said he must not be taken to say that it was actually the shape of a horseshoe, because one side was nearly straight.

Asked if he would not expect to find at right angles to what he thought was originally the site of the navel, the tendinous intersections, Mr. Pepper said, No; they might not be found, because tendinous intersection did not involve the skin; it only involved the muscles underneath, and there the muscles had been cut away.

Asked if there was any trace of the linea alba in the remains in the tray, Mr. Pepper replied that the linea alba was not on the surface of the skin; it indicated where the tendons underneath were joined. One never saw the linea alba on the chest. It was underneath the surface.

Asked if there was not enough there for the linea alba to exist if the flesh really did come from the lower part of the abdomen, the witness said, "No, because there is only one muscle on one side there."

Asked if very great dexterity would not be required to remove the peritoneum, Mr. Pepper said he did not know what Counsel meant; he never saw any one attempt to remove it. There was no trace of the peritoneum there. He agreed that it must have been removed, but not by itself; it was simply left with some of the fat; the body had been cut up, and it had been taken out evenly in the whole thickness, with considerable quantities left behind, as shown by the preparation. It was there originally underneath that skin. It was not there now.

Asked whether, if it was there, that would be a good indication that that piece of skin really came from the lower part of somebody's abdomen, the witness said he very much doubted if one would recognize it clearly in this condition, but assuming one could see it, of course it would tell one that it was from the abdomen. It was removed during the cutting up of the body. Instead of pieces being taken out at right angles they were cut obliquely.

Asked by Counsel if on that piece of skin some hair was visible, the witness answered, Yes. He had expressed the opinion that those were pubic hairs. They were on one side only of that piece of skin. They were on the right side, as on the body.

It was put to him by Counsel that, if those were pubic hairs, those on the left side must have fallen out. To this the witness answered, Yes. He agreed that if they had fallen out on the left side there might have been hairs over other portions of that piece of skin that had also fallen out. There was only one place where pubic hairs were found in this substance, and that was in a line at the bottom.

In reply to further questions, the witness said the removal of the navel was exceedingly common.

Asked how many times he examined that piece of skin before he came to the conclusion that it was a scar, he said he came to the conclusion immediately—at least, within a quarter to half an hour at his first examination. He had not the slightest doubt it was a scar.

In reply to further questions, he said that on July 15th he and Dr. Marshall examined the remains at the mortuary for two hours and three-quarters, or three hours. On that occasion he did not see the mark. He could not remember when he heard of the operation. He did not agree that the condition of that piece of skin made it very difficult to say whether that mark was a scar at all.

Asked about the horseshoe depression, he said that the left limb of that depression was undoubtedly due to a folding of the skin. The scar began at the lower part, where it was cut across. It was practically straight, slightly curving at the upper part.

Asked by the Lord Chief Justice if that was as high as the navel would have been, or not as high, or above it, he said the navel varied exceedingly. The scar being 4½ in. long, one might easily allow an inch for contraction; that would take it to 5½ in.

Asked if that would be the distance between the navel and the pubic bone, he said he did not know the stoutness of the woman or her age, and it depended upon whether one started from the surface or the bone.

Further questioned by Counsel for the defence and by the Lord Chief Justice, Mr. Pepper said his contention was that the scar ran on the right side, nearly straight, and that the left limb and the curve of the so-called horseshoe were formed by the fold. He had examined microscopically a small piece that was cut out of the centre of what he said was the scar. He had examined the right side with the microscope. There were on the right side of what he called the scar little groups of transverse lines, four lines in each, at regular intervals.

Asked if he had ascertained whether or not those little groups of four lines in each, which he admitted were partly in the area of the so-called scar and partly outside it, were exactly similar to the pattern on the elastic material of the underclothing, he said he could not say whether or not the patterns precisely corresponded.

Asked whether, if they did, that would be a clear indication that those marks were caused by the pressure of the materials, he replied that they might on the left side, but not on the right. The scar was not a line; it was ⅜ in. wide at the lower part. The marks in groups were on the left side of that.

The Lord Chief Justice: He says that those marks might be caused at that side by the pressure of the combinations.

Counsel: I understood you to say that those marks are in the area of the so-called scar and travel outside it on one side?—I was just looking; I do not think they go beyond; it is only in a certain light that you can see them. (After examining) I should say they do not go beyond the area of the scar.

You think they do not?—They do not.

Are you examining it now with the microscope or not?—No.

You have not examined it for that purpose with the

microscope at all?—Yes, I have examined a piece which was cut.

Asked if, supposing one limb of the horseshoe mark was due to folding and the other side of it due to an operation, and, therefore, a scar, would he not expect that if a piece was cut across the folded depression and a piece cut across the area of the so-called scar, the cut edges of the piece cut from the so-called scar would be different from the cut edges of the piece of the groove where there never had been a scar at all, Mr. Pepper replied: "Not necessarily, because the folding over would attenuate the skin at that part—the continued pressure of it." Both limbs of the so-called horseshoe were cut across.

Asked if he would not expect the cut edges of that piece that were originally within the scar to be different in appearance from the cut edges of the skin just outside the area of the so-called scar on each side, Mr. Pepper said: "Not necessarily, because the superficial area of the skin has entirely gone, and this has become hardened and horny; the whole skin on the surface is almost like leather now." He went on to say, in reply to further questions, that he himself had examined, both with the naked eye and with the microscope, the bit of skin cut by his directions across and beyond the area of the so-called scar. In the part which he said was the scar the fibres were more densely placed than the fibres forming the skin. A very important point was that there were glands of the skin still remaining in the skin on each side of the scar; there were no remains of glands in the part which he said was the scar. An operation would not necessarily cause an alteration in the size, number, and arrangement of the fibres in the part immediately below the surface. A wound going through the entire wall of the abdomen of a woman might unite very accurately indeed, so that afterwards one could not tell really that an operation had been done, except from the line of the scar. That was not a very unusual thing. There must be new tissue which united the two edges of the wound, of course; one could not get a scar if there was not new tissue; but it might unite, and frequently did, so accurately, that a long time after the operation one could not tell, except by the line on the surface, that an operation had been done. If the scar was not stretched there was a continuous white line. If a scar was stretched, it did not necessarily stretch in a lozenge shape—wider in the middle. A scar at the lower part of the abdomen often did not, because the tendons were frequently separated during the operation, allowing the scar to be wider at the lower end than at the top; it would be somewhat triangular in shape. Asked what the measurement of this so-called scar was, the witness said ⅛ in. at the lower part, ½ in. at the middle, and ¼ in. near the top—narrowest at the top and widest at the bottom. It was exactly the scar that he had many times seen after an operation in this part.

Asked whether, in the majority of cases, if there had been an operation he did not expect afterwards to see marks of the stitches, the witness replied, "In the majority of the cases, yes; in many, no." Asked if there were any marks he could clearly identify as stitches in this case, the witness said, No; there were some marks which were doubtful, and, therefore, he was quite ready to admit that they were not marks of stitches. They suggested stitches, but did not prove it; but where there was a scar which had become broad like that, very often the stitch marks became merged in the margin of the scar. Even apart from that, stitch marks frequently disappeared altogether.

Re-examination.

The presence of the aponeurosis proved that the part came from the lower abdomen in front.

At the request of the Lord Chief Justice, Mr. Pepper described the arrangement of the muscles with the aponeuroses. The reason he said the piece was not above the navel was because there were none of those transverse markings he described, and, further, there were pubic hairs.

In reply to questions by Mr. Muir, he said his knowledge, or want of knowledge, as to the date of Mrs. Crippen's disappearance had no effect whatever upon the opinion he formed as to the time those remains had been in the ground, nor had the information that an operation had been done on Mrs. Crippen any effect on his opinion that a scar was present.

Proceeding, he said the fact that the kidney found weighed about 2¼ oz. was, in his opinion, due to loss of moisture. This loss of moisture would be very largely due to the presence of the large quantity of lime there; that would abstract the moisture from the parts. Absolute quicklime destroyed flesh by abstracting the moisture, becoming converted into hydrate. The presence of damp clay round quicklime would tend to convert quicklime into slaked lime. It would make the action of the lime less strong. Where there was a moist ground the moisture was abstracted by the lime. When it became slaked it did not corrode flesh; a great deal of carbonate of lime was formed, and the carbonate of lime facilitated the formation of adipocere. With regard to the protection or otherwise of the tissues, the slaked lime would practically be inert.

Asked if it was or was not possible that the navel might have been outside the area of that piece of flesh, Mr. Pepper said it was quite possible. The distance varied in individuals. In his experience it had varied from about 4½ in. to 6 in; of course in a very tall person or a very stout person, if one took the curve it might quite easily extend to more than that. He thought it most probable that it was within the area of that piece of skin—possibly beyond it. The part of the wall of the abdomen containing the tendinous intersections was not present. Their absence from that piece of flesh did not in any way tend to show that it was not a part of the wall of the abdomen. He was confident that it was part of the wall of the abdomen, and of the lower part of the front of the abdomen. The same observation applied to the linea alba. He had performed many hundreds of abdominal operations. The scar which he found there was such as he had found in his actual practice.

Asked if it would be possible, examining these remains on September 9th, to form an opinion of any value as to how long they had been in the ground—supposing he then examined them for the first time—the witness said he could not have formed an opinion of any value, because at that date they had been largely exposed to the air, and the ordinary putrefactive process—decomposition—had attacked them.

Now, that piece you have before you has been preserved?—It has been preserved from the very date I discovered it.

In reply to further questions, he said he examined the section made by Dr. Spilsbury, and found no glands in the part he said was a scar. There were glands on each side. There were also glands in the left limb of the so-called horseshoe where the fold was.

The Lord Chief Justice said there were two or three questions he wished to ask. First, whether this was a scar, and, secondly, as to its position. As to its being a scar, he understood the witness to say that it was nearly 1 in. wide at the bottom. Mr. Pepper replied that it was ⅝ in., and it tapered up something like ¼ in.

Asked if that was in accordance with his experience as to the shape of a scar when it became a dried and old scar in an abdominal operation, the witness replied in the affirmative. It was bigger at the bottom than at the top; this was the case repeatedly in this situation.

The Lord Chief Justice proceeded: Do you generally find, when it does take place between the navel and the pubic region, that that is the shape the scar assumes?—Frequently.

Now I want to ask you, in operations is the navel ever removed?—Oh, yes; I have done it many times myself.

Therefore, the presence or absence of the navel after the operation will not be conclusive one way or the other?—No, my lord.

You have no doubt that this is a scar?—None.

Will you tell the jury why it was between the navel and the pubes?—In the first place, the scar is wider at the bottom than at the top; and, secondly, there is this line of hair which, in my opinion, is pubic hair.

The pubic hair goes higher up in some individuals than it does in others?—Oh, yes.

Do you see any indication of hair in that specimen above the region which you think is the pubic region?—No, my lord.

Therefore it would confirm your view that it has reached down to the pubes where the hair grows and has gone up towards the navel?—Yes.

Is there any indication of that being a piece of flesh which came from above the navel?—I say absolutely it is not.

The Lord Chief Justice: What is the operation in which the middle line between the navel and the pubes is the seat of the scar—the common operation?—The most common is operation for removal of the ovaries or uterus.

What is the operation in that position which is performed on a male subject?—Removal of stones from the bladder—taking tumours from the bladder.

It would be that class of operation in male subjects?—Yes; it is frequently performed there on male subjects.

Then the scar there would be of the same appearance?—Yes; it would be less likely to be so wide in a male subject, because, as a rule, there is not so much distension.

The width at the bottom would point more to a female than to a male subject?—Yes; the scar usually stretches more.

In reply to further questions, Mr. Pepper said that the part which contained the tendinous intersection was not present. When he spoke of "slicing" he meant vertically. The upper part was cut, so that the muscle was cut lower down than the skin. Asked by the Lord Chief Justice to say on what he based his opinion that the remains could not have been in the ground longer than eight months and not less than four months, Mr. Pepper said he should have thought they might have been in the ground for eight months if he had paid attention to some parts where the decomposition or change was much more advanced; but, looking at certain parts of the skin and the heart, and the kidneys and the liver, he should have said that, if anything, the time was under four months; they were so exceedingly well preserved. Looking at the general conditions of the organs as they were, he came to the conclusion that they could not have been buried more than eight months.

DR. BERNARD EDWARD SPILSBURY.

Crown Evidence in Chief.

Dr. Spilsbury, Pathologist at St. Mary's Hospital, said he had on several occasions examined the piece of skin and flesh, the first time on September 9th. It came from the lower part of the wall of the abdomen, near the middle line. He based that opinion upon the presence and the arrangement of certain muscles. There was the rectus muscle and an aponeurosis, attached to which were other pieces of muscle. Confirmation of his opinion was afforded by the existence at the lower margin of the piece of a row of short, dark hairs. He made a section across the middle of what he regarded as a scar and examined it under the microscope. There was no epidermis on it, but he found a small mass embedded in it at one spot. This indicated to him the line of incision of the skin at the operation which caused the scar. At each end of the section on either side of the scar there were glands, but there were no glands in the scar itself. On the other hand, there

were glands in the groove which had been described as a horse-shoe mark on the skin. The tissue at the place he had called a scar was denser than elsewhere; it was undoubtedly an old operation scar.

Cross-examination.

He commenced his medical work at Oxford University, and went to St. Mary's Hospital about eleven years ago. Mr. Pepper was a lecturer there then, and he had been associated with him for the first five or six years, but not since. The person who removed the viscera must have had considerable dexterity and considerable anatomical knowledge, and must have been accustomed to evisceration. If it were established that there was in the scar a sebaceous gland or a hair and a hair follicle it would be conclusive that it was not a scar. He thought the mark was a scar, even when he looked at it with the naked eye. He believed he had already heard, when he first saw the skin, that Belle Elmore had had an operation. It was more difficult to tell if the scar were a scar when he saw it than it would have been had it been fresh.

Re-examination.

He was only associated with Mr. Pepper by attending his lectures, and acting as a surgical dresser. That fact had absolutely no influence on his opinion; nor did hearing of an operation having been performed on Belle Elmore.

You have expressed the opinion that beyond doubt this is a scar?—I have.

You also said that if any gland or hair follicle exists in that place it will conclusively prove that it is not a scar?—Yes.

Have you examined microscopically the section which you took out?—I have.

To see whether there is any gland or any hair follicle in it?—I have.

He had searched the scar carefully for glands or hair follicles, and must have found them if there were any. There was nothing which could conceivably be mistaken for a gland except the small mass of included epidermis which he had mentioned.

Asked to explain more fully what he meant by included epidermis, he said that in a surgical operation, when the edges of the skin were brought into contact it was common for at least one side to turn in a little, and as the scar formed some of the surface material covering the skin might become enclosed in the scar and embedded in it. There was in his opinion no room for doubt that the mark was a scar. It was quite possible for the inturned edge of the cut to contain both hair follicle and sebaceous glands, though there were none in this instance. The arrangement of muscle and aponeurosis he had described was inconsistent with the piece of flesh and skin being from any part of the body except the lower abdomen. He had the microscopic slides, and would send for a microscope and show them if it were really desired.

Dr. Thomas Marshall.

Crown Evidence in Chief.

Dr. Marshall, Divisional Surgeon of Police for the Kentish Town District, described the fashion in which he had collected the remains with Mr. Pepper. He had heard the latter's evidence as to what took place on that occasion, and agreed with it. He was with Mr. Pepper on August 8th, and saw the piece of flesh bearing a mark. He formed the opinion that it was from the lowest part of the abdominal wall. On the same occasion he noted the mark and regarded it as a scar. In none of the organs found could he see any indication of disease which might cause death.

Cross-examination.

He agreed that some bodies remained in an excellent state of preservation for some years if buried in lime and in a soil like clay which practically excluded all air, and that the possibility of giving a certain opinion as to the length of time that a body had been buried in the earth depended on many circumstances. Before he examined the skin and flesh he had heard that there had been an operation. It was on August 8th he noted the scar, and that was the first time he had examined the skin and flesh. Before he had merely handled it and other remains when taking them from the cellar and placing them in jars, and for other purposes. He had then seen the skin, but not examined it. He expressed his opinion as to the length of time the remains had been buried at the inquest before any other witness and without consulting Mr. Pepper. He placed the time at several months. All he could say further was that it was just possible that the time was as long as seven months.

Dr. W. H. Wilcox.

Crown Evidence in Chief.

Dr. Willcox, Senior Analyst to the Home Office, said that on July 22nd he received from the coroner's officer five jars covered and sealed which he numbered forthwith. In the first was a small portion of liver and one kidney. In the second, a pair of combinations. In the third, hair and a hair-curler, a handkerchief, undervest, and some hair in a piece of paper. In the fourth, a piece of pyjama jacket, and in the fifth two other pieces of pyjama jacket. One piece had a button on it and a neck-piece bearing a tab. These pieces were of flannelette, as also were the two complete suits handed to him for a second inspection in court. The buttons were also the same, though one was a little smaller, having shrunk perhaps a little. It was a circular button with a depression in the centre, from which threads radiated. On July 25th he received from Dr. Marshall another jar, which he numbered 6. It contained some intestine, another curler with hair in it, and a portion of liver, which, together with the other portion, completed the whole liver. On August 8th he received from Mr. Pepper a piece of skin. It was from the lower wall of the abdomen, and showed a horse-shoe mark, of which one limb was a scar—an old scar—and the other limb was a fold. On August 14th he received from Dr. Marshall a seventh jar containing some soil and lime, and the same day an eighth jar containing lungs, a portion of intestines, a piece of muscle, and another piece of hair. He also received a box of carbolic powder. The lungs were in a condition of advanced putrefaction when received. The kidney which he had received much earlier was comparatively fresh, except that it had undergone the process of decay with the formation of adipocere, but there was very little ordinary putrefaction present in it when he received it. The greater putrefaction in the lungs was due to the fact that a longer time had elapsed between their removal from the ground and their being brought to him. Organs removed from burial in the ground and exposed to a warm atmosphere decomposed and putrefied very rapidly. On August 15th he received an unopened bottle of Neville's sanitary fluid, and on the 16th an open bottle containing a smaller quantity. On August 23rd he visited 39, Hilldrop Crescent himself, and procured some specimens of the soils from the excavation, which he put in jars and labelled.

Continuing, Dr. Willcox said that on July 23rd he commenced examining the stomach, the kidney, and a portion of the liver, first looking for mineral and organic poisons. He found traces of arsenic in the intestines and liver, and traces of creosol (which was the chemical name for commercial carbolic acid) in the stomach and kidney, and intestines and liver—small traces. He attached no importance to those. They were due to the disinfectants. He began the same day to examine for alkaloidal poisons. Such an examination required two or three weeks before the final tests could be applied. He took weighed portions of the stomach and intestines and kidney and liver, and treated them by the usual process for extraction of alkaloids, and found an alkaloid present in all the extracts.

Did you then apply further tests to see what kind of alkaloid was present?—Yes, I tested for all the common alkaloids—morphine, strychnine, cocaine, and so on—and I found that a mydriatic alkaloid was present; that is, an alkaloid the solution of which if put into the eye of an animal causes the pupil to enlarge and dilate.

What are the mydriatic alkaloids?—Having found a mydriatic alkaloid I applied a further test and found that it was a mydriatic vegetable alkaloid. There are three mydriatic vegetable alkaloids: atropine, hyoscyamine, and hyoscine. I found that one of those three was present. I applied further tests and found that the alkaloid that I got in the extracts corresponded to hyoscine.

The Lord Chief Justice: You have no doubt it was hyoscine?—No.

Counsel: How were you able to tell that it was hyoscine?—In two ways; one by examining the residue with a lens and microscope; it was gummy; there were no crystals there. Another way was by adding to a solution of the residue some bromine solution—hydrobromic acid—and I got round spheres but no crystals. Hyoscine gives spheres exactly like I have got. Atropine and hyoscamine both give needle-shaped crystals.

The Lord Chief Justice: The two things you have described—the gummy residue and the spheres got from the bromine solution—pointed to hyoscine only?—That is so.

Asked if he could say how much there was, Dr. Willcox replied, Yes; in the stomach one-thirtieth of a grain; in the one kidney one-fortieth of a grain.

That would be in the part that you analysed?—No, calculated out on the whole organ.

Calculating on what you found, there would be in the whole of the organ, how much?—One-thirtieth in the stomach, one-fortieth in the kidney, one-seventh in the intestines, one-twelfth in the liver.

Any in the lungs?—There was the merest trace.

What was the total amount of hyoscine in all the organs submitted to you?—Two-sevenths of a grain.

Is hyoscine used medicinally in the form of hyoscine?—No; it it is gummy, syrupy stuff, which it would be impossible to handle, and a salt is used. The salt which is used is the hydrobromide of hyoscine. That is the preparation given in the *British Pharmacopœia*.

Are you able to estimate how much hyoscine hydrobromide there was in the whole body?—In the whole of the organs submitted to me the amount was two-fifths of a grain of the hydrobromide.

And in the whole body?—It would certainly correspond to, in the whole body, more than half a grain.

Would that be a fatal dose?—Yes.

What is a fatal dose?—From quarter to half a grain.

Asked what were the effects of hyoscine hydrobromide if taken, Dr. Willcox replied: It is a drug which is a powerful narcotic poison.

The Lord Chief Justice: I think what Mr. Oddie wants you to give us—very shortly, because we do not want to get a medical lecture—is, What are the effects on the subject? If a dangerous dose is given, what course will it take? You say it is a powerful narcotic; what then?

Dr. Willcox replied: If the fatal dose were given it would perhaps produce a little delirium and excitement at first; the pupils of the eyes would be paralysed; the mouth and throat would be dry; and then quickly the patient would become drowsy and unconscious and completely paralysed and death would result in a few hours.

Continuing, Dr. Willcox said the time within which the drowsy and unconscious state would be reached depended on the amount given and on the condition of the stomach. Assuming to have been given the dose which he thought he could trace, the drowsy, unconscious state might come on in probably under an hour, and paralysis and death in hours.

What do you mean by hours—four or five?—Probably something under twelve hours.

The Lord Chief Justice: Would the patient recover at all during that twelve hours?—No, not if the dose was a fatal dose.

Assuming a fatal dose, there would be the dryness of the mouth and so on, then unconsciousness, and then death, all in one continuous sequence, without recovery?—Yes, my lord.

Continuing, Dr. Willcox said hyoscine was not a commonly used drug. When used it was, if given internally, administered practically always by hypodermic injection. It was used as a powerful sedative for cases of delirium, mania, meningitis (which meant, he explained in answer to the Lord Chief Justice, inflammation of the brain), also for delirium tremens, and very occasionally as a hypnotic for insomnia; sometimes it was given in combination with morphine for sedative purposes. In all these cases it was given hypodermically. The proper dose for hypodermic injection was from a two-hundredth to one-hundredth of a grain. As far as the witness knew, hyoscine was not used as a homœopathic remedy. He had looked through the English and the American homœopathic pharmacopœias, and the drug was not mentioned. He had formed an opinion that the drug was taken in this particular case by the mouth. It was rather salt and bitter, but it could be administered without the patient knowing if given in something with a pronounced flavour and sweetish taste such as starch, beer, stout, spirits, sweetened tea, or coffee.

Counsel: What was the cause of death in this case in your opinion?—Poisoning by hyoscine.

Counsel: Do you know of any legitimate use for hydrobromide of hyoscine except in the doses you have mentioned?—No, not for internal administration.

Cross-examination.

This was the first case where the question of murder by hyoscine had arisen. He had tested for hyoscine before, but had never found it in extracts from dead bodies before this case. There were both vegetable and animal mydriatic alkaloids. The latter were produced after death by the action of bacteria when the organs had reached an advanced stage of putrefaction. He did not discover in the remains sufficient alkaloid to apply the melting point test. He identified the alkaloid as hyoscine about August 20th.

Had you on August 2nd been informed that Dr. Crippen had bought some hyoscine?—Yes.

So that eighteen days before you formed the opinion that it was hyoscine you had been told that Dr. Crippen had bought some?—Yes, that is so.

Now I want to go through your process in the very plainest language that I can. I want to know the process of testing, first of all, to find whether there was any alkaloid in the body at all, next the process to ascertain whether such alkaloid was mydriatic, and, thirdly, the process to ascertain whether such mydriatic alkaloid was vegetable or animal. I want those three stages. Take the first stage. How many days would it take to ascertain whether there was any alkaloid at all?—It would take about a fortnight.

In testing to see whether there was any alkaloid at all, you first, I suppose, weigh the different parts of the remains where it is supposed that an alkaloid might possibly be?—Yes.

And you mix those up quite fine, I suppose?—Yes.

And place them in rectified spirits of wine?—Yes.

Then after they have been for how long in spirits of wine do you drain off the spirits of wine?—Twenty four hours.

And then you place what is left of the mixed-up flesh in another lot of spirits?—Quite so.

You draw that off after another twenty-four hours?—Yes.

And so on?—And so on, as long as the liquid which comes away is coloured—about five times.

Then when the liquid ceases to get coloured you stop?—Yes.

You went through that process, and finally you found that there was an alkaloid?—Yes, there are several other stages to the process; if you would like them I can give them.

I do not think I want them; anything you think fair you will tell me, I know. Now of that substance which you found in this first process, how much did you find in the liver; was it one-twelfth of a grain?—Yes.

And in the intestines?—One-seventh.

In the stomach one-thirtieth of a grain?—Yes.

And in the kidney one-fortieth of a grain?—Yes, that is calculating out on the whole of the organ; I did not find that amount.

That is the opinion you formed?

The Lord Chief Justice: What he said was, Calculating from what he did find in the organs it gave that result.

Counsel: We will call them the calculated results. So that there was comparatively little in the stomach and kidney as compared with what you found in the liver and intestine?—There was less.

Now the second stage. Having ascertained that there was an alkaloid, next you tested to find out whether it was mydriatic. The physiological test would be conclusive on that point, would it not?—Yes.

Pouring a drop into an animal's eye, and finding that it dilated the pupil?—Yes.

Is that the test you used?—Yes; I used cats. I put a drop of the solution into a cat's eye, and then exposed the cat to a very powerful light. One pupil was widely dilated.

Of course, I am accepting what you say up to this point. On the physiological test you found the solution mydriatic; it was quite conclusive because it paralysed the pupil?—Yes, with all the four extracts, the stomach, the intestines, the liver, and the kidney.

Therefore you have ascertained now—first that it was an alkaloid, and secondly that it was a mydriatic alkaloid?—Yes.

I think you have told us that mydriatic alkaloids may be either vegetable or animal?—Yes.

The three vegetable mydriatics that you specified were hyoscine, hyoscyamine, and atropine?—Yes.

Is there a fourth vegetable mydriatic—cocaine?—Yes.

And perhaps there are several other vegetable mydriatics?—Yes, there are, which have slight mydriatic action. May I explain as regards the cocaine, it is not quite the same as the other three.

Tell me why?—Cocaine causes the pupil to enlarge, just as the others do, but if the eye is exposed to a powerful light the pupil contracts; it does not paralyse it.

Then we can quite fairly eliminate cocaine?—Yes, I think so.

Then it comes to this; the three main vegetable mydriatics are hyoscine, hyoscyamine, and atropine?—Yes, I think those are the only three we need consider.

Hyoscine and hyoscyamine being produced by the plant called henbane, and atropine from belladonna?—Yes.

Have those three vegetable mydriatics, namely, hyoscine, hyoscyamine, and atropine, the same chemical composition and exactly the same chemical formulae?—No; up to a few years ago it was thought they had, but in the *British Pharmacopœia*, the last edition, a different formula was given for hyoscine, and all the recent work on these alkaloids points to the fact that hyoscine has a slightly different formula from the other two.

The Lord Chief Justice: You say up to a certain time all three were the same; lately, through further researches, hyoscine has got a different formula?—Yes.

Counsel: When was it that they were discovered to be different?—About eight years ago, I believe.

Now, tell me what is the difference in the formula as ascertained now, in the light of the latest research, between hyoscine on the one hand, and the other two on the other.—Hyoscine $C_{17}.H_{21}.NO_4$, and atropine and hyoscyamine C_{17} H_{23} and NO_3.

In hyoscine, in the light of the latest research, 17 parts of carbon?—Yes.

And in the other two, hyoscyamine and atropine, 17 parts of carbon too?—Yes, the same.

As regards hydrogen, the latest research shows 21 parts in hyoscine and 23 in hyoscyamine and atropine?—Quite so.

The Lord Chief Justice: Is not the difference pretty plain; it is four parts of hydrogen and one part of oxygen; that is right, is it not?—No, my lord; two parts of hydrogen and one part of oxygen.

Counsel: The difference is two parts of hydrogen and one part of oxygen. Until eight years ago it was always recognized that those three had the same chemical formula?—Yes. I will not be certain about the eight years; it is somewhere about then.

Do you know for how many years it had been recognized in the profession that the formula for all three was exactly the same?—For a good many years—a number of years.

Tell me next, do putrefying bodies give off carbon?—Yes.

Do putrefying bodies give off hydrogen?—Yes; they give off compounds containing those elements.

And do putrefying bodies give off compounds containing nitrogen?—Certainly.

Therefore, to that extent, the constituents of a vegetable mydriatic alkaloid exist in animal mydriatics?—The constituents exist, but the combination is different.

I am now going to the processes that you applied in order to find out which mydriatic it was. Did you apply what is known as the Vitali test?—Yes.

Is that a test by which at last you arrive at something which is coloured violet and turns to dark red?—A purple violet colour is produced which fades.

Fades into dark red?—It fades; I would rather not have the dark red in.

The Lord Chief Justice: Purple violet colour produced which gradually fades away?—Fades away to a brownish colour.

Counsel: Is the arrival at that point (violet fading away to a brownish colour) characteristic of mydriatic alkaloids, both vegetable and also animal?—No; certainly not with the animal. It is with the vegetable.

Do you know this book, *Hamilton and Bodkin's Legal Medicine?*—No, I am not familiar with it.

You have heard of it, I suppose?—I am not sure that I have.

Have you heard of Giotto and Spiecke?—Yes.

They are celebrated Italian chemists or analysts?—Yes.

Are they recognized as high authorities?—I believe they are well known chemists.

Have you read that according to Giotto and Spiecke certain bromo-atropines do give Vitali's reaction?—I have read that; but I have looked through the work of these gentlemen, and I have been unable to find any record of it. I do not accept the statement. I know that it is present in another book in addition to the one you are reading from, but I have been unable to find the statement in their original papers. Even if it is in them, I do not accept it, because it does not agree with my experience.

The Witness, having agreed with counsel's statement that the

Vitali reaction was common to hyoscine, atropine, and hyoscyamine, was asked whether the important test between them was not the melting point. In reply, he said: It is *an* important test, but not *the* important test. It is a test which can only be applied when one has a considerable quantity of the alkaloid to deal with.

Which do you consider the most important one?—The careful observation, with a lens and a microscope, of the alkaloid itself as to whether it is crystalline or syrupy—gummy—and also the bromine test which has already been mentioned—the obtaining of crystals. This gumminess was characteristic of hyoscine, while hyoscyamine and atropine were crystalline.

You arrived at the gumminess when you found out that it was an alkaloid, but before you had applied any test to find out whether the alkaloid was mydriatic?—Yes, that is so.

In other words, before you applied the physiological test to the cat's eye?—Yes.

Then gumminess, I gather from you now, was the result of these extractions, which resulted in your ascertaining that it was an alkaloid?—Yes.

Supposing that the alkaloid had, in fact, been hyoscyamine or atropine, might not gumminess have been the result, too, at this stage?—I think that most probably there would have been some crystals there.

But might there not, in fact, have only been gumminess?—There might possibly, but, on recrystallization, crystals would have appeared.

Gumminess. Now, that most possibly, if hyoscyamine or atropine, would be crystalline?—Yes.

Continuing, Dr. Willcox said the gummy extract could not possibly be hyoscyamine or atropine if the extracts were fairly pure. If the extracts were impure and had other materials in, then a gummy residue might have been obtained with atropine and hyoscyamine. He had applied the bromine test after the Vitali test—the test for vegetable mydriatic alkaloid. The bromine test was only of value in discriminating between the three alkaloids in question. Other things besides hyoscine gave the spheres of which he had spoken.

Further questioned as to the value of the melting-point test, Dr. Willcox said that if a sufficient amount of alkaloid could be obtained the test was a valuable one, but in toxicological work such an amount never could be obtained. It would mean an enormous dose.

Will you agree that if you could get enough it would be the most valuable test of all?—No, I would not agree with that.

Do you agree that the different mydriatic alkaloids have a melting point of widely different degrees?—Yes, providing that they are in a very pure condition.

Is the melting point of hyoscine 65°?—Yes.

Counsel: 65° Centigrade?—I will accept your figures.

I do not want you to accept them unless they are accurate. Hyoscine is 65° Centigrade; do you accept that?—I would like you to give me the three together, please.

I will put all three together. The melting point of hyoscine 65°, that of hyoscyamine 105°, that of atropine 115°?—Yes, those are right, except with regard to hyoscine; it is a gummy syrup, and it is very difficult to say what the melting point is. I should prefer to leave a stroke against that.

Approximately 65°; would that be right?—Well, it is very difficult to say when it does melt; it is a syrupy-looking stuff.

Then you accept hyoscyamine 105°, and atropine 115°, but you do not accept hyoscine 65° because it is difficult to tell?—Yes.

The Lord Chief Justice: Would not the gumminess get more liquid so that you could approximately tell?—It is very difficult to tell.

You agree that it is lower, but you say it is very difficult to fix the point because with the gummy substance you cannot tell when the point is reached?—Exactly.

Counsel: But you agree that the melting point of hyoscine is lower than that of the other two?—Certainly.

The witness was then questioned as to the gold chloride test, the alkaloid being dissolved in diluted hydrochloric acid, chloride of gold added, and crystallization allowed to take place. He agreed that it was a valuable test as the melting points of the different crystals were very different, atropine 148°, hyoscyamine 160°, and hyoscine 199°. He would also agree that it was perhaps the most valuable differentiating test between the three alkaloids, but always providing there was sufficient alkaloid for its application.

Re-examination.

The gold chloride test was the first process he had contemplated applying, but he found his solutions were not strong enough, and that if he applied it he would waste all his material unsuccessfully. In a poisoning case there was never enough material for the melting-point test in his experience. For the purpose of a poisoning investigation the tests used must of necessity be tests which applied to very small quantities.

Are the tests which you have applied real tests?—Oh, yes.

That is, you can absolutely distinguish this substance from the other substances that you were looking for?—Yes; I tried all these tests on the pure alkaloids themselves before I applied them to extracts from the viscera, and found them reliable and that they gave corresponding results.

Now, as the result of your tests are you able to say whether this substance was an animal alkaloid or not?—It was not an animal alkaloid.

You are able to say that to your own satisfaction?—Yes.

You are quite satisfied?—Yes.

You have told us that mydriatic animal alkaloids are produced in decaying bodies at an advanced stage of putrefaction. What was the most advanced in putrefaction?—The lungs were much the most putrefied of the organs.

Did you test them for animal alkaloids?—I tested them most carefully.

Did you find any at all?—The least trace—not enough to paralyse the pupil, but just to weaken it.

Further questioned on this point, Dr. Willcox said that the traces of mydriatic alkaloids in the lungs were so slight that he could not say whether they were animal or vegetable, and had excluded them from his calculation of the amount of alkaloids present in the organs examined.

You were asked about this process of extraction being long and elaborate, and you said it took about a fortnight; have you been in the habit of applying this test?—Oh yes, a very great many times. In the last ten years I have done it considerably over a hundred times.

Does it present any difficulties which you are not accustomed to deal with?—No.

When you have extracted this alkaloid, is it possible to confuse the gummy residue of the one with the crystalline residue of the other?—Not if you have a pure residue.

And did you have a pure residue?—Yes.

Have you ever found in all your investigations a ptomaine, that is to say, an animal alkaloid, which gave a purple colour with the Vitali test?—No, I have tested several hundreds of times, and recently on some viscera several years old, and I have been unable to obtain it in any single case.

Animal alkaloids never correspond to Vitali's test?—No, I have never found one, and I have tested specially for that hundreds of times.

The Lord Chief Justice: Of course a considerable portion of your time is occupied in this kind of investigation; I mean, it is no novelty to you?—Oh no.

You would in the first instance test for animal alkaloids; and you have sworn that you did test to find animal alkaloids in this, if there were any?—Yes.

What I want to ask is this. You told Mr. Tobin, very fairly, that they are produced in what you call the advanced stages or late stages of putrefaction. Apart from your examination, were there any organs in such a stage of putrefaction that you would expect to have alkaloids produced?—No; not those that I examined for alkaloids first. The lung, which I examined two or three weeks later, was in such a state that there might have been some animal alkaloid there.

In that you found only a trace of any alkaloid—not sufficient to test whether it was animal or vegetable?—Yes.

Have you any doubt that it was vegetable alkaloid that you discovered, and that it was hyoscine also?—No.

Dr. A. P. Luff.

Crown Evidence in Chief.

Dr. A. P. Luff, Physician to St. Mary's Hospital, said that for seventeen years he had been Scientific Analyst to the Home Office and resigned about six years ago. He had heard Dr. Willcox's evidence and had repeated his tests with pure drugs. They were absolutely the right tests, and he agreed with Dr. Willcox's results. The poison present was undoubtedly hyoscine, judging by those tests. He had often tested for animal mydriatic alkaloids in every toxicological case during his period of office, but had never found them in a corpse. Before that he had conducted a long series of investigations upon animal alkaloids, and had once found a mydriatic animal alkaloid in some meat which he had put to putrefy in circumstances very favourable to putrefaction. He had applied the Vitali test to the mydriatic found in this meat, and it proved negative. The distinction between the animal and vegetable mydriatic alkaloids was absolute if the Vitali test was applied.

Cross-examination.

Cross-examined, Dr. Luff said that he had written a textbook on forensic medicine and toxicology, which was published in 1895. There had been no subsequent edition. It contained a passage to the effect that it was difficult to state positively the age of remains owing to variations in the depth and fashion of burial, the cause of death, the length of time between that event and burial, and differences in different bodies. He still held that view.

Re-examined, he said that he had not seen the remains in this case, and had not asked to do so for a very good reason. Remains changed so rapidly, that he could not have given an opinion on their age; but he could have done so if he had seen them when Mr. Pepper saw them, and nothing in his book invalidated that statement.

THE DEFENCE.

Dr. G. M. Turnbull.

Defence Evidence in Chief.

Dr. G. M. Turnbull said he was Director of the Pathological Institute at the London Hospital, and also a member of the Pathological Society of Great Britain; admission to it was by vote. The Pathological Institute was the largest in the United Kingdom. Under his supervision in 1907, 1908, 1909, the average number of *post-mortem* examinations was 1,251, and under his supervision complete microscopic investigations were carried out. He devoted his time to the work and the microscopical examination of material sent down by the surgeons. He had seen the piece of skin and flesh on September 9th and on October 15th and 17th. On the first occasion a cut had been made from one side to the other of the horseshoe mark,

including the so-called scar. Three slices had also been made. His examination of them enabled him to say that what was called a scar could not possibly be one. The grounds of his opinion were that he found certain structures which had never been found in a scar before. They were two groups of hair follicles, in which the hairs were visible in cross section. There were also in two of the sections in relation to these hairs a small piece of a sebaceous gland, and another large piece of the same sebaceous or fatty gland in another section. A bay of subcutaneous fat could also be found within the area described as a scar. What was described as a scar was due to the skin having been folded over and something having been between the fold, and thus producing pressure, which dried the skin at the fold. In a cut across what the prosecution called a fold and what was called a scar he found the same horny appearance, whereas the appearance of one and the other ought to be different.

Cross-examination.

Are you able to form an opinion as to what part of the body the skin and flesh under discussion comes from?—I cannot say definitely, but I have a very good idea.

From what part of the body does it come?—I think that the best explanation is the lower part of the abdomen.

The Lord Chief Justice: If you suggest anything else, do so. If you agree that it comes from the lower part of the abdominal wall, there is an end of it.—Well, after that last examination, I considered that the origin from the lower part of the abdomen was the most satisfactory.

Then you do agree that it comes from the lower part?—I will not disagree.

Counsel: But if you are going to suggest any other part of the body, say so. I am most anxious to get this quite clear. You say the best explanation is that it comes from the lower part of the abdomen. Do you, with your great experience, agree that it does come from the lower part of the abdomen?—I will admit——

I am not talking about what you admit. You are not here to make admissions. You are asked to say, with your great experience, to the best of your honest judgement, from what part that was taken?—Then I would say the abdomen.

The Lord Chief Justice: Please remember that you are not here as an advocate but as a witness, and the word "admit" is not the right word to use.

Counsel: When you first examined that piece of flesh did you form a different opinion as to the part of the body it had come from?—Yes.

Did you express that opinion in writing?—May I answer your last question again? When I said, "Yes," I should have said——

Did you form an opinion that that came from a different part of the body?—No; I did not give a definite opinion.

Did you put your opinion in writing?—I did.

The witness then said he had not been present during the evidence of the medical witnesses on the other side. It was not quite because he did not wish to be present; he was not asked to come.

Then it was not by your wish; which do you mean?—It was not by my wish, but I think I had said at the very beginning, when I was asked to undertake this examination, that I hoped it would not mean having to give evidence.

Did you give your first opinion on that understanding?—I had been promised that I would not be called at all as a witness.

You gave your first opinion upon the understanding that you would not be called as a witness?—No, I would not say that. I went to see these remains on that understanding. Before I gave my first opinion I was promised I should not be called as a witness.

The medical witnesses for the prosecution had been cross-examined on what he had stated in his report to the defence. He only knew of the reasons they had since given for their opinions from seeing a copy of the depositions.

You did not hear the questions put?—No.

And you did not hear the answers given?—No.

You had the running record, which is neither question nor answer, which appears on the depositions, to guide you as to what reasons they gave for their opinions?—I had the officially signed depositions, I believe—a copy—and I had an account sent to me.

After you had seen their answers to the questions put upon your opinion, did you alter your opinion?—Not because of any such questions—oh!—it must be so.

Please answer my question.—I altered my opinion considerably after the second time on which I examined that piece.

That is not an answer to my question. I asked you whether you altered your opinion after you had seen the reasons they gave for their opinions?—Yes, it must have been after that.

In your opinion which you first gave, did you state that the aponeurosis was absent from that piece of flesh?—No; I said that I thought there was an absence of the aponeuroses characteristic of the abdomen.

The Lord Chief Justice: That is not the same thing as saying that aponeurosis was absent. If the aponeuroses characteristic of the abdomen are absent, are there aponeuroses there or not?—There are aponeuroses there, or were there, my lord.

Counsel: Your opinion now is that this is part of the abdominal wall—the lower part?—Yes.

Do you find there the abdominal muscle?—I find——

Will you answer the question, Yes or No? Did you or did you not find the abdominal muscle there?

The Lord Chief Justice: You shall add anything you like, but just answer the question first.

Counsel: Did you find there the abdominal muscle?—Which abdominal muscle?

The rectus abdominis?—Yes.

You did?—I think so.

Is that muscle in life attached to the pubic bone by a tendon?—Yes.

Do you find there part of the tendon which had attached that muscle to the pubic bone?—I had not seen that.

Do you see it now?—No.

(Dr. Spilsbury was then asked to point out to Dr. Turnbull what was suggested to be the tendon in question, and did so.)

The Lord Chief Justice: Now, what is your answer?—It is not where I should expect it from the dissections I have made.

You are asked whether you can see the presence or traces of the presence of the tendon which attaches the muscle to the pubic bone. Your last answer is that the abdominal muscle is attached to the pubic bone by the tendon. Do you say that it is there or not?—No.

You say it is not there?—I do not think this is it.

You say you do not find it?—There is a tendon there, and, if the muscle is the rectus, it is.

Please answer this one way or the other; it is most important. Do you find that tendon there or not?—Yes.

Have you any doubt that this piece of skin is part of the abdomen?—Yes.

You have?—Yes.

The Lord Chief Justice: Do listen, Dr. Turnbull.

Counsel: What part of the body do you suggest it comes from if not from the abdomen?—I have told you, but I do not think you can have a better explanation.

What part does it come from if not from the abdomen?—From the upper part of the thigh.

The Lord Chief Justice: I am in a great difficulty here. I understood this witness to say in examination in chief that in his opinion this piece of skin did come from the abdominal wall. That was the best suggestion he could make. I think now we must proceed on the hypothesis that it does come from the abdominal wall, in accordance with the evidence of the other witnesses.

Counsel: Now, will you kindly read me what you said in your original report about this piece of flesh not coming from the abdominal wall?—"We are of opinion that that skin does not come from the abdomen for the following reasons." (The reasons given were that it was too coarse and the sebaceous glands were too large and too prominent; that the structure of the muscles and the arrangement of their fibres would not be easily accounted for, and that the aponeurosis was not present. The term meant Dr. Wall and himself.

And you now tell us that the characteristic aponeurosis is present in that piece of skin?—Yes.

The Lord Chief Justice: What length of examination had you and Dr. Wall made of this specimen before you wrote that report when you said that there was no aponeurosis present?—I should say twenty minutes, my lord.

Counsel: Do you find there some transverse muscles on the left side on the deepest surface of the specimen?—Yes.

The fibres running in two directions, one superimposed on the other?—In one direction, certainly.

Do those correspond with the fibres of the internal oblique muscle and the transversalis muscle of the abdomen?—They could, I think.

The Lord Chief Justice: You are asked do they correspond with what would be shown by the internal oblique muscle and the transversalis muscle of the abdomen?—It is really impossible to say that.

The Lord Chief Justice: What did you mean by saying that they *could* correspond?—I said they might correspond, but they are so altered that you cannot see.

Counsel: If they correspond would not that show beyond question that they came from the abdominal wall?—Yes.

Do you know the method of stitching the abdominal wall used in America?—I have got some fourteen methods down.

The method by which the stitches do not come to the surface except at the beginning and at the end?—There are several methods.

So that there would be only one stitch there if that method of operation were used?—Yes.

Did you find the mark of a stitch there?—I did not.

Is it a common thing for part of the epidermis to be enfolded when the abdominal wall has been divided and sewn up again?—No; not common.

Have you seen it happen?—Yes.

In how many cases have you seen it happen?—I could not say; very few. The surgeon always takes some instruments and turns the edges up to prevent that after he has put in the suture.

I put it to you that it is not at all uncommon?—I do not agree.

It does happen?—It can happen.

And does?—And does; yes.

You have seen it yourself?—I have seen the edges turned in.

The Lord Chief Justice: The result of the edges being turned is that a bit of the epidermis gets enfolded or below the folds of the scar?—I have only read of that. I have never seen it.

So that you are not familiar with the appearance of such a case?—Yes, but not from operation; from accidents it is very familiar.

The Lord Chief Justice: Are you familiar with the appearances of a scar where the epidermis has got enfolded below the

folds of the scar?—Yes; but not an operation scar—an accident scar. The appearances are similar.

How do you know if you have never seen them?—I have read about them.

I am asking you whether you are familiar with the appearance of an operation scar where part of the epidermis has been enfolded in the wound?—I have never seen that at all.

Then you cannot tell me what takes place when such a wound heals?—Yes, I have seen the phenomena when, for instance, a man has fallen upon his hand here and cut his hand, causing an incision like an operation, and it has been allowed to heal, and he has got such an included epidermis—from an accidental incision, not an operation incision.

That is the nearest you have ever come to seeing such a case?—Yes.

Now, I put it to you that the inclusion of epidermis in the incision made for the purposes of an operation might be easily mistaken for sebaceous glands?—For one it might be, but not easily; I disagree from what I have read, and I think we must learn. It might be mistaken for one sebaceous gland, but not for more.

The Lord Chief Justice: I do not quite see why if it may be mistaken for one it may not be mistaken for two, if there happen to be two included in the piece of flesh?

What Mr. Muir is putting to you is that where you get the flesh included you might find one or more sebaceous glands in between the lips of the scar.

You have said that it might be mistaken for one sebaceous gland but not for more. We do not understand what you mean by that. What is it?—I know the appearance of these inclusions because I know that the accidental ones are similar to those in operations. I have never seen one in an operation scar, but I have read, of course, about them; and such an inclusion might be mistaken by somebody unaccustomed to the microscope.

Counsel: We are not talking about people unaccustomed to the microscope. We are talking about people like Dr. Spilsbury; that is the person I am talking about. Do not talk about people unaccustomed to the microscope. I am talking about people accustomed to the microscope, and I am suggesting to you that persons accustomed to the microscope might be deceived?—No, I will not agree with that.

Is epidermis composed of cells called epithelial cells?—Yes.

May those cells be mistaken for sebaceous glands?—I think not.

Or for hair follicles?—Yes; the cells binding hair follicles have a similar appearance.

The Witness introduced with his further answers statements that follicles with hair in them could not be mistaken for epithelium, and finally the Lord Chief Justice pressed upon his notice that he was not being asked about the appearance of hair, but that of the follicular epithelium.

Counsel: Might the one be mistaken for the other if included in a clean scar?—Would you repeat the question to me.

No sir, I won't. The horny appearance you say is due to drying?—Yes.

When did you see it first?—The first occasion we saw it was on September 9th.

It had been out of the grave since July 14th?—Yes.

You never saw it before that?—No.

Might it have dried between these two dates?—No—the skin might have dried, yes.

During re-examination Dr. Turnbull said that from start to finish he had never wavered in his opinion that the mark was not a scar.

The Lord Chief Justice: I want to ask you two questions. You said in your first report that you and Dr. Wall were of opinion that the aponeuroses so characteristic of the abdominal wall were not present. When did you change that opinion?—I had done a great many dissections and had macerated certain portions of the abdominal wall to see whether these aponeuroses would lose what I thought were their characteristics.

I am not asking you that; I am asking you when you changed your opinion?—I definitely wrote a different opinion after the second examination.

I asked you when you changed your opinion. Can you give me a date?—On October 15th.

Up to that time you had let the defence be under the opinion that what you thought was the absence of aponeuroses was conclusive against its being the lower part of the stomach?—No, I had simply said——

Do attend. Had you told them that you had changed your opinion before October 18th?—No.

Up to that time you had let the defence be under the impression contained in your first opinion?—Yes—oh no, I beg your pardon—I had asked if he could not see the specimen again to confirm our opinion.

What I really want to know is this. You have read out a statement in your report that the skin and attached flesh came not from the lower part of the abdomen but from what you call the root of the thigh. When did you first communicate to the defence that that opinion of yours had changed?—On October 15th.

You have now made it plain, but put your hand on your body and indicate where you mean by the root of the thigh.—Just here.

Below the rump, so to speak?—Yes.

The Lord Chief Justice having asked Mr. Pepper to go to the witness-box and show Dr. Turnbull the dimensions of the scar which he had mentioned in the course of his evidence, began to question Mr. Pepper, but first asked Dr. Turnbull if he had himself seen old scars of various operations, and received an affirmative reply.

The questions then put to Mr. Pepper resulted in his stating that at the bottom the scar measured $\frac{7}{8}$ in. About $1\frac{3}{4}$ in. higher up it measured $\frac{1}{2}$ in., and $\frac{1}{4}$ in. at the top of such part of the scar as the skin found showed.

Pins having been inserted by Mr. Pepper at the places mentioned by him, the Chief Justice said: Now, Dr. Turnbull, I will repeat the question that I put to you. Do not assume that that is the whole of the mark, but there is what is pointed out to you there. Is it in accordance with your experience that the mark of a scar from ovariotomy is wider at the lower part of the abdomen than higher up?—No.

That is not in accordance with your experience?—No.

Now you were not here in court, but have you read that one of the witnesses here said that he had performed many hundreds of operations, and that the scar, as a rule, was wider at the bottom than it was at the top?—I read that, but do not agree with it. He added that he had never performed any operation himself, but had examined ovariotomy scars but could not give their number. In his experience it was untrue to say they were generally wider at the bottom than at the top.

Now does the fact that there is that difference of breadth in what I will call a mark affect your judgement at all or not?—Yes.

To what extent does it affect your judgement?—If it is said to be a stretched scar I should have thought it would be stretched to its widest in a different position.

I am talking about the scar on the lower part of the woman's stomach?—It might be stretched 1 in. The fact that the mark was wider at the bottom was, in his opinion, an argument against its being a scar.

Now, I want to ask you this, if you please. Do you say that a mark of that kind could be caused by folding—that is to say, a mark which is wider at the bottom and narrows up?—Yes.

You think it could?—Yes.

The Lord Chief Justice: That is all I have to ask.

Dr. Reginald Cecil Wall.

Defence Evidence in Chief.

Dr. Wall said he was M.A., M.D. of Oxford, F.R.C.P.Lond., and M.R.C.S. He also obtained the Fitzgerald Exhibition, which, in answer to the Lord Chief Justice, he explained was awarded after an examination in classics, and had nothing to do with medicine. He had also obtained the Andrew Clark prize in medicine and pathology at the London Hospital. He was an assistant physician there, and at the Brompton Hospital for Consumption. Until the beginning of this year he was one of the pathologists at the London Hospital, and was one of the examiners in materia medica at the Apothecaries' Hall. He was also a Fellow of the Royal Society of Medicine and of the Medical Society of London, and was the author of various medical works. For two years he had been Demonstrator of Physiology at the London Hospital. He had seen the piece of skin and flesh in question, first on September 9th, secondly on October 15th, and thirdly on October 17th. He was present when the incision was made across each limb of the so called horse-shoe, the site of what was said to be a scar, but not when the pieces were removed for examination. His examination of the skin on October 15th commenced at 11.30 and finished at 4.15. It was not a microscopical examination, simply with the eye and a hand lens. At the second examination on October 17th he and Dr. Turnbull saw the piece of skin for a short time to identify certain points that they wanted to confirm, and the remainder of the time they were present they were examining the microscopic sections prepared by Dr. Spilsbury. The time spent over that examination was a little over two hours.

As the result of his examination he concluded there was no scar.

Asked for the reasons he said: I could not see on inspection by the naked eye or with the hand lens such an appearance as I should have expected to find if there had been a scar in that situation. I found appearances which I could explain much more easily on the supposition that the skin had been folded in that region. Secondly, after the incision which had been made by Mr. Pepper very kindly, I did not on examining the cut surfaces of the edges of the skin find such an alteration in structure as I should have expected had there been originally a scar, and on comparing the cut surface at the site where the scar was alleged to be I did not find that the appearance of the cut surface differed from the appearance of the cut surface of the other part of the groove where it is admitted there is no scar.

Asked to explain what differences he would have expected to find, he said: That is a very different question to answer. The different new tissue may assume various appearances in different circumstances. All that I should have expected would be that it presented a different appearance — not the same appearance. He added that on examination with the naked eye he saw no structures which could not occur in a scar, but on microscopical examination he observed in the region called a scar traces of hair follicles, five in number, and a sebaceous gland, outside that region there were other hairs and other sebaceous glands.

Dr. Wall was then asked to explain what he meant when he had spoken of folding and to show the jury how the appearance of folding might have occurred, and did so. A number of questions and answers followed, from which it appeared that Dr. Wall thought the skin must have been rolled twice, and

that between part of the fold there must have been some substance, some fabric, and that something must have prevented the skin unrolling.

The Lord Chief Justice: We want to follow this evidence closely; it is very important on this particular part of the case. You say to produce what you have described it must have been rolled over twice, and you said accidentally rolled over. Would it remain in the rolled-over position if it was put or thrown down without something being on the top of it?—No, I do not know it would have.

If rolled over twice, as has been described, what would keep it in that position until the superincumbent weight comes upon it?—It might have been rolled over by something falling upon it.

Twice?—Well, if we throw a cloth into a basket it will roll over a good many times.

You think it could have been rolled over twice by something?—I think it is very easy to explain it in that way.

Dr. Wall was then asked whence he derived his information as to the evidence given in the case. He said he had had a copy of the police-court depositions sent him. As for the present trial he had had the evidence sent to him.

The Lord Chief Justice: Do you mean a shorthand note of it, or what?—I have had a copy of the evidence sent to me. I suppose it was taken from the shorthand notes.

The Lord Chief Justice: We must find out what was sent.

Dr. Wall then said: I had a print taken from a newspaper—the *Daily Mail*.

Cross-examination.

You have been dependent, have you, for the medical details given by those gentlemen who were called as expert witnesses in this case for the prosecution upon a newspaper report?—I am afraid I did not quite catch what your question was. Have I been dependent for what?

For the medical details given by the other witnesses?—Yes, I have.

Dependent on a newspaper report?—Yes.

Have you had no other means of knowing what the reasons were that they gave for their opinions?—No.

Were you a party to a report before any of those experts were cross-examined at the police-court?—Yes.

Did you hear any of the medical experts for the Crown give their evidence at the police-court?—No. The only medical witness that he had heard examined or cross-examined was Dr. Turnbull. He agreed with him practically entirely. There was no essential detail in which he disagreed, nor any detail at all. He had seen in the depositions sent to him the following statement: "If Dr. Wall and Dr. Turnbull say that there is an entire absence of aponeurosis, I should say it is due to forgetfulness or defective observation." It was a question professedly put in consequence of that report.

He was then questioned as to whether the words used in the deposition and the words used in the report did not amount to the same thing. Counsel suggested that to say that the aponeuroses characteristic of the abdominal wall were not present was the same thing as saying there was entire absence of the aponeuroses of the abdominal wall. But the witness did not agree. He would admit, however, that since the report was drawn up they had modified their opinion as to the absence of the characteristic aponeurosis. At the time he was asked to make an examination of the remains, nothing was said as to his giving or not giving evidence. There was a further examination at the joint request of Dr. Turnbull and himself, after they had read the evidence of Mr. Spilsbury, Mr. Pepper, and Dr. Willcox. They were asked to give evidence on October 15th. This was after they had made the second examination, but before they had made the third. They made a fresh report, a verbal report, not a written one.

Counsel: Withdrawing anything you had said in the first report?—Certainly.

What was it you withdrew?—We said we were not so certain that a piece of skin did not come from the abdominal wall.

What is your opinion now?—My opinion now is that it may have come from the abdominal wall.

What is your opinion as to where it comes from?—My opinion is that there is not sufficient evidence to say definitely where it comes from; I think it does come from the abdominal wall, probably.

The Lord Chief Justice: Now, Dr. Wall, can you suggest at all where it could have come from except the abdominal wall?—The two parts of the body from which it may be derived, as we thought when we first saw the piece of skin, were the lower part of the abdomen and the upper part of the thigh on the inner side.

And you did (quite rightly I dare say according to your then lights) report for the purpose of cross-examination that it did not come from the abdominal wall?—We reported in our opinion it probably did not come from the abdominal wall.

That is the opinion you have modified?—Yes.

With all your present lights with what you now know about the aponeurosis have you any doubt that it did come from the abdominal wall?—I cannot say I have no doubts.

DR. ALEXANDER WYNTER BLYTH.

Defence Evidence in Chief.

Dr. A. W. Blyth, in answer to questions, said he was a Member of the Royal College of Surgeons and a Fellow of the Institute of Chemistry, and had various other qualifications, including Fellow of the Chemical Society. He was the author of a work entitled *Poisons, and their Effects and Detection.*

Dr. Willcox told us yesterday that going through the ordinary processes he at last extracted a gummy substance. Now, is a gummy substance characteristic of hyoscine and not of hyoscyamine, atropine, or any animal alkaloid?—Certainly not. You can have a gummy substance in extracting various alkaloids. Often the slightest impurity—especially with regard to hyoscyamine—causes it not to crystallize. By the term gummy substance I presume is meant something that is not crystalline—a sticky substance.

The Lord Chief Justice: Quite right. Dr. Willcox said hyoscyamine might crystallize, hyoscine did not; do you agree with that or not?—I do not agree—emphatically.

Counsel: Do you say that hyoscine may crystallize?—Hyoscyamine may crystallize.

I asked you as to hyoscine?—Hyoscine is very difficult to crystallize under any circumstances.

The point is this. It is said that atropine crystallizes and hyoscyamine crystallizes, but hyoscine does not. Do you agree with that or not?—I do; that would be in a pure state. He added that he knew that Vitali's test was characteristic of the vegetable alkaloids, but had no experience of a mydriatic alkaloid obtained from animal tissues. Whether those alkaloids reacted to the test or not he did not know. No one knew.

It does give the purple colour in the three vegetable mydriatic alkaloids but you have no experience of its giving it in animal alkaloids?—Yes.

Dr. Willcox said that small round spheres were produced. Are those round spheres characteristic of hyoscine alone, or are they also found with hyoscyamine and atropine?—I have not been able to get them. I have attempted to get what Dr. Willcox has stated according to the depositions that have been forwarded to me, but I must confess that I have not been able to distinguish between the atropine, hyoscyamine, and hyoscine by hydrochloric acid, as Dr. Willcox has done. Possibly he manipulated it in some particular way, but——

The Lord Chief Justice: You have not been able to do it?—I have not been able to do it.

Counsel: Does any one know whether those round spheres might be produced at last in the case of animal alkaloids?—No.

Dr. Willcox told us that in the lungs, which were most decomposed, he found a trace only of any alkaloid—so small a trace that he could not say if it was animal or vegetable. If it was animal, would you expect that he would find most of the animal alkaloid in the most decomposed part, the lungs?—I should not have expected so, because animal alkaloids arise, it is well known, at a particular stage of putrefaction, and when that stage is passed any animal alkaloid that has been produced becomes more or less destroyed, so that in the same decomposing tissue at different times of its putrefaction you would never expect to find the same amount. It has gone a stage beyond the time of production.

Cross-examination.

Have you ever tested a mydriatic animal alkaloid?—Never.

Have you read Dr. Luff's evidence?—Yes. I have read all the evidence.

Have you read that he has himself found a mydriatic animal alkaloid in putrefied meat?—Yes.

And has tested it?—Yes.

And that it did not give the purple colour on Vitali's test?—Yes.

Do you agree with that or not?—Oh, I dare say he is quite correct. I do not dispute for a moment that there may be many mydriatic alkaloids.

The Lord Chief Justice: This gentleman has said he had no experience of animal mydriatic alkaloids.

Counsel: Then you are speculating at large?—I do not know about speculating.

Do you know of any animal mydriatic alkaloid except metaline?—That particular one was investigated, but there are others that have not been thoroughly investigated.

The Lord Chief Justice: Do you know of any other?—I know of none by specific name, except under the name of mydriatic alkaloids.

Counsel: Is not that the only mydriatic animal alkaloid that you can give a name to?—Well, one was separated in 1869 by Sonschein, and he gave it no name, but it seems not to have agreed with metaline.

Can you give any name to any other animal mydriatic alkaloid but metaline?—No, I cannot, because they are unnamed.

Have you any experience of them at all?—Not practically.

In your opinion, is it possible to make a mistake between animal mydriatic alkaloids and vegetable mydriatic alkaloids?—I think the evidence points that they are the same thing.

Would you mind answering my question?—Is it possible to make a mistake between the two?—You would like me to answer "Yes" or "No," but that would not be fair. In my opinion some of them are identical, therefore it is possible to make a mistake between the two.

Mr. A. Wynter Blyth's attention was then drawn to the 1895 or last edition of his book. On p. 485 appeared a statement that when the existence of ptomaines first became known it was feared they might interfere with the detection of poisoning cases, but further research had conclusively shown that no ptomaine was known which so closely resembled a vegetable poison as to be likely in skilled hands to cause confusion. Was this a correct statement?—No, not absolutely so. I have altered my opinion since I wrote that book.

Have you published that altered opinion?—I have not; I have not had an opportunity yet.

Not in any paper read before any society?—No.

When did you alter your opinion?—I have altered it lately.

How lately?—During this month, in reading up the various papers—foreign papers.

For the purposes of this case?—In connexion with this case, of course, obviously.

For the purposes of this case you have altered your opinion?—I have, yes; I think there is strong evidence that there is in putrefying tissues a substance very much resembling the mydriatic alkaloids produced.

Did you read that Dr. Luff said this: "I think it quite impossible to mistake animal for vegetable alkaloids if Vitali's test is applied"?—Yes, I have read that.

Was it since you altered your opinion that you read that, or before; he said it yesterday?—What did you say?

Was it since you altered your opinion that you read that? He said it was impossible to mistake vegetable for animal alkaloids if you properly applied Vitali's test.—Oh, I disagree with that entirely, because some of the Italian chemists declare that they have got different reactions.

Who are those Italian chemists?—I forget their names; there is one Giotti.

Have you searched their original papers?—I have not; I have only seen extracts.

Dr. Willcox has said that he searched the original paper, and that statement is not to be found in it?—Well, I cannot help that; I have not searched for it; I have not had time.

The Lord Chief Justice: Can you refer to any book, up to any date you like, which says that animal alkaloids will give the purple colour under Vitali's test?—I have no book.

Speech for the Defence.

In the course of his speech for the defence, Mr. Tobin said that one thing needed for the murder was a dextrous hand, well versed in anatomical operations. Dr. Crippen never conducted a *post-mortem* examination in his life. The Crown had not been able to suggest he had any dexterity in anatomy. Proof positive was also needed that these remains had been buried since 1.30 on February 1st of this year. Dr. Marshall, you will remember, said Mr. Tobin, told us that in such conditions as the conditions obtaining in this case remains might keep in an excellent state of preservation for some years. Proof positive, continued Mr. Tobin, is also lacking that the remains were those of a woman. The anatomical grounds for such a conclusion, Mr. Pepper told you, were wholly wanting.

The Crown case is that Belle Elmore underwent an operation and that there was a scar, which was found upon the flesh. There is also, admittedly, a mark caused by the folding of the skin. Are you persuaded that the other mark was not caused in that way, too? Dr. Turnbull has told you he sees with a microscope structures which could not have been there if the mark had been a scar.

Then there is the hyoscine. Dr. Willcox says he found traces of a vegetable alkaloid. May not the traces have been those of an animal alkaloid produced after death, as a result of putrefaction? Has science probed these matters to the bottom? Eight years ago the chemical formula for the three vegetable alkaloids was the same. Who knows what further research may not bring?

Speech for the Prosecution.

Mr. Muir, in the course of his speech for the Crown, said: No medical man can say with certainty on anatomical grounds that those remains are the remains of a woman. Further, anatomical grounds are the only grounds upon which, as medical men, they would be justified in forming any opinion at all.

The flesh has been produced here, and I observe how bravely you did your duty in regard to that ghastly exhibit. I would not have shirked the ordeal if I could have been of any assistance to you, but fortunately I had the assistance of my friend, whose help is much more valuable than mine, and I was spared the duty. (Mr. Muir was here referring to his junior, Mr. Oddie, who has studied medicine in addition to law.) I sympathized with you in having to do it, and admired the way in which you did it. That piece of flesh was one of the most important things, perhaps the most important thing, in the case.

Was that piece of flesh a piece of the flesh of Belle Elmore? It was said by the witnesses for the Crown to be a piece of the lower abdominal wall, and to bear an old scar. Belle Elmore had been operated upon in that region in the year 1892 or 1893. It was disputed by my learned friend, in opening, that the flesh was part of the abdominal wall. He suggested that his medical evidence would leave your minds in such doubt as to that being a part of the abdominal wall of any human being that you would be bound to acquit his client. What is the history of the evidence on that point?

I can summarize it in three sentences. Mr. Pepper, a surgeon of the greatest experience, who in practice in London has himself performed hundreds of operations, and has seen the scars, after they have healed, which have been caused by these operations, said they were just such scars as this—wider at the bottom, narrowing towards the top. Dr. Willcox, a surgeon of great experience, and Dr. Marshall, the police surgeon, also saw this flesh when it was fresh, before it had been preserved, as it had to be preserved, in order that it might be produced in this court.

All three saw it. All three say it is a scar. In the witness-box at the police-court they were faced with cross-examination based upon the report of Dr. Turnbull and Dr. Wall, that that piece of flesh did not come from the abdominal wall at all, but that it came from another part of the body. These two men were obliged to admit in that witness-box that they were absolutely mistaken. Yet these are the two men who did not scruple to give a report of a twenty minutes' examination of a piece of human body to the effect that three distinguished colleagues had been talking nonsense in the witness-box on oath.

Their report having been put to Mr. Pepper, and cross-examination having been based upon it, we have recantation—recantation and confession of incapacity, of the most gross carelessness and rashness on the part of these two gentlemen. These two men, who have so recanted and have so confessed, are the two men upon whose evidence you are asked to say that Mr. Pepper, with his vast experience, and Dr. Willcox, who, acting for the Home Office, has probably conducted more of these inquiries than any other living man, except, perhaps, Dr. Luff, are absolutely mistaken.

Dr. Turnbull has examined the piece microscopically, so have Dr. Spilsbury, Mr. Pepper, and Dr. Willcox. All Dr. Turnbull can do is to say, in answer to me, that a person unaccustomed to the use of the microscope might make a mistake about it. As though I were talking about a person unaccustomed to the use of the microscope! Did he mean to suggest in that innuendo that Dr. Spilsbury was unaccustomed to the use of the microscope? Was that the suggestion? I hope not. You did not see the things in the microscope, because they would not convey to your minds any definite or certain impression, and so you are left to judge between Dr. Willcox, Mr. Pepper, and Dr. Spilsbury on the one hand, and Dr. Turnbull on the other, by the light of what you know of Dr. Turnbull's competence and caution and with regard to that twenty minutes' inspection and that joint report with Dr. Wall.

It was suggested that this was not a scar at all—that it was caused by a fold. You have had the advantage of having the specimen and the surgeons before you, and seeing and judging for yourselves the weight and the authority of the evidence upon that point, and I do not propose to detain you further than simply to point out the mistakes which Dr. Turnbull made. He was wrong as to the part of the body from which this flesh came. He was wrong as to its not having the characteristic aponeuroses of the abdominal wall. He was wrong as to the absence of the tendon and the transverse muscles. All these mistakes he has had to confess in the witness-box. You will judge upon which, the evidence for the Crown or the defence, you can rely.

As for Dr. Wall, I never heard anything more extraordinary in the witness-box in my life than what he was pleased to call his reasons. "If it were a scar," he said, "I should have expected to find something different from what I did find." Nothing more than that, to put against the reasoned evidence of Mr. Pepper, Dr. Willcox, and Dr. Spilsbury. Is that a piece of the lower abdominal wall of a human being? Your answer, in my submission, must be "Yes." Has it got a scar upon it? Your answer is the answer which you have formed upon your careful examination of this piece of flesh, and that is a matter for you. Is it a scar upon the body of a woman? The evidence of Mr. Pepper is emphatically yes, because there is that stretching at the bottom of the scar in a woman which does not take place in the case of a man. Then if a woman—if a scar at the lower part of the abdomen—from whose body did it come?

As for the age of the remains, medical men, exercising the skill and knowledge that they have, who saw those remains at the time they were disinterred, express the opinion that they were buried from eight to four months. What is the suggestion on the part of the defence?

It was said that the prisoner had not the opportunity or the skill necessary to the burial and mutilation of those remains. As to his skill in anatomy or his lack of it, you will have to decide upon the evidence which has been given. His diploma, signed by a professor of anatomy or surgery, showed that he passed or satisfied the authorities of the college upon the subject. There is his own statement that he attended hospitals in London before taking his degree with the purpose of witnessing operations. There are the endorsements on his diploma showing he was practising in different cities in America, which qualified him to practise in all the branches named on it. Mr. Pepper told you that once a man learnt how such removals were done it would not matter whether he practised it for ten years or not. It is not a delicate task which requires constant skill and practice to do it with accuracy. It does require medical knowledge, and some degree of dexterity, such, says Mr. Pepper, as would be acquired by going through the medical school, and would not be lost by ten years of want of practice.

I won't detain you by going at any length into the question of the finding of the hyoscine. What is said by my learned friend, even to the last? It was unfortunate, he says, that Dr. Willcox knew, before he found the hyoscine, that the prisoner had purchased hyoscine. Having made that suggestion, and having said in the same breath he was making no suggestion, what else has he got to set up against the evidence of Dr. Willcox and Dr. Luff?—Dr. Wynter Blyth.

Gentlemen, scientific witnesses, so-called, sometimes make sorry spectacles of themselves in the witness-box, but was there ever a sorrier spectacle than Dr. Wynter Blyth in that box? Having read to him from his own book this passage, "Further research has conclusively shown that at present no ptomaine is known which so closely resembles vegetable poison as to be likely in skilled hands to cause confusion," he says that since he has been instructed in this case he had altered that opinion, upon the authority of some supposed statements of two Italian analysts, which supposed statements Dr. Willcox had investi-

gated, and found that, so far as the original papers went, they had never made the statement at all! That is the evidence on the question of hyoscine, Dr. Wynter Blyth against Dr. Luff and Dr. Willcox, and I do not think I should be justified in taking up your time in going again through the tests which were applied to discover whether this was hyoscine or not, except for this one purpose—to point out that my learned friend, Mr. Tobin, is mistaken in what he addressed to you in regard to Vitali's test. Vitali's test has nothing whatever to do with the brown spheres. It is the test which distinguishes between animal and vegetable alkaloids.

A vegetable alkaloid, says Dr. Luff, gives a purple colour with Vitali's test; an animal alkaloid does not. Dr. Luff has himself experimented with the test, the object of which is to distinguish between the two classes of alkaloids. The bromide and crystalline tests distinguish between the three vegetable alkaloids. Atropine and hyoscyamine form crystals like common table salt with the bromide test; hyoscine gives a gummy residue, like syrup, and forms the spheres instead of crystals. Dr. Willcox says his extracts were pure, because he repeated the experiments with pure alkaloids, purchased from the chemist, and they gave the same results. The only suggestion made by Dr. Blyth that, if there was some impurity, there would be difficulty in getting a crystal deposit from the two others falls to the ground.

SUMMING UP.

The Lord Chief Justice, in the course of his summing up, said that it was not now seriously disputed that the remains were those of a woman; but it was true that Dr. Willcox, or one of the witnesses for the Crown, said with very great candour that a scar would not indicate a man or a woman, because you might have a scar in that place—the middle of the stomach—upon a man.

After indicating the reasons for and against the belief that the remains if those of a woman were those of Belle Elmore, the Lord Chief Justice said he now arrived at the battle-ground in the case, the piece of flesh. I am going to remind you of what the dispute is. For that purpose I will very briefly take the evidence of Mr. Pepper and Dr. Turnbull, who are the two rivals. Mr. Pepper says it undoubtedly comes from the lower part of the abdomen of a woman. It is not to be forgotten that when cross-examining at the police court on the instructions of the report of Dr. Wall and Dr. Turnbull, Mr. Newton, the solicitor for the defence, was instructed that it never came from the abdomen at all, and you heard in court yesterday from Dr. Turnbull and Dr. Wall the reasons for that statement, which they then made with every confidence, that as there were no characteristic aponeuroses it could not be abdominal.

Now, take those two witnesses. I say nothing about the way in which they gave their evidence. It is always unpleasant to criticize expert witnesses. You saw the difficulty they were in. It amounted to this: they could not suggest any part of the body from which it could come consistently with what they found there, except the lower part of the abdomen, and the characteristic answer was given by Dr. Turnbull, which I will read to you from my own note: "The aponeurosis is split in the way it is found in the lower part of the abdomen." Mr. Pepper and the other witnesses for the Crown say that the piece which they examined has got the aponeurosis in it, and that it came from the lower part of the abdomen. That is not now disputed.

Now is that a scar or not? There has been a little misunderstanding about this part of the case, and I want to make it plain to you. It is not now disputed that Belle Elmore had a scar in that place due to an operation for ovariotomy, which means taking out the ovaries. It extended roughly up to, or near, if not as far as, the navel. It was originally 6 in. long, and subsequently contracted. In addition to the suggestion made about the poison, and having heard that poison had been found, it was also suggested before the Crown witnesses made their examination they knew that she had a scar. If they did, it was perfectly right that they should know it, because they had to look for what would identify the body, and I cannot understand what was meant by the suggestion that it was "unfortunate."

Now, you are dealing with the piece of flesh, which is very different from what it was when it first went into the ground, and all the medical men agree that it would have been much easier to judge if the piece of flesh had been seen sooner. The medical men for the Crown saw it sooner, but not much sooner. Mr. Pepper says—and he is supported by Dr. Spilsbury, Dr. Marshall, and Dr. Willcox—"This is a scar similar to that which I have seen over and over again on the stomach or abdomen of a woman who has had this operation done; as a rule, with women it is wider near the bottom than it is towards the top, and it presents exactly the appearances that Belle Elmore's operation would present." Further than that, they said: "I examined it for sebaceous glands and for hair follicles, and in the scar, properly so called, there are none." Further, Dr. Spilsbury said there is enclosed (I think it is called) in the scar a piece of epidermis that has come from the side.

Now, remember, at the time this was before the police court in the first instance the scar was unimportant from the point of view of the defence, because their case then was, "This is not the abdomen at all." When they come into court here they, for the first time, develop this theory. They say, if you examine the scar you will find in the scar, properly so-called, one sebaceous gland and four or five hair follicles. When Dr. Turnbull is cross-examined as to the possibility of there being an included piece of epidermis in the scar he says, "I have read of this happening, but I have never seen it." The witnesses for the Crown tell you that they have seen it repeatedly. You will judge as between these witnesses which is right. It is frankly admitted by the Crown that if in an ordinary piece of scar there are sebaceous glands and hair follicles, that is not a scar proper; but they point out to you that you may have that appearance locally in a part of the scar, and, rightly or wrongly—it is entirely for you—the witnesses for the Crown swear that there are no sebaceous glands. Gentlemen, you and I had a further examination yesterday. There was one remarkable piece of evidence given—not contradicted by Dr. Turnbull, although I do not say he assented to it. Mr. Pepper said you can see under the magnifying glass in that scar, practically speaking, the irregular line of the knife from top to bottom. Now, if that is so, gentlemen (you will be able to judge when you come to look at it again), we need not trouble much more about it.

Mr. Tobin, the counsel for the defence, made a point of this—and he is quite entitled to ask your consideration to it—that on that piece of flesh there is no navel. The witnesses for the Crown admit that the muscle went up as far as the navel and probably round it, but say they think the navel has been removed from that piece of skin. Mr. Tobin, I think, three or four times said to you that Mrs. Martinetti said she saw the woman's navel, and Crippen's evidence—though I am afraid you must take it with some reserve—was that his wife had a navel. You will have to ask yourselves, if a woman looks at another woman's stomach casually and there is a scar which excites her attention which comes up to the position of the navel—navels are a different shape; sometimes there are projections, and so on—do you think what has been said to have been seen by Mrs. Martinetti is sufficient to contradict the evidence which you have heard on the other side if it is satisfactory to you?

Now, I do not propose to read again at length to you the reasons given by the defendant's two witnesses for saying that it is not a scar. I remind you again that they were prepared up to October 7th to say that there was no aponeurosis at all; that there was nothing upon the skin or flesh which would enable you to put the piece of flesh into its place upon the body; that it came from the thigh or from the buttock.

When they are cross-examined they entirely abandon that position, and say the best opinion they can give now with all their examination is that it came from the place in which it was sworn that they came from by the witnesses of the Crown, namely, from the lower part of the abdomen. Gentlemen, you will, of course, consider all that evidence, but you must regard with some reserve the evidence of a man who has given such positive evidence that it was not from the abdomen, and has abandoned that, and has fallen back upon there being no scar.

If the appearance of the one sebaceous gland and the five follicles of hair is due to the fact that there was an included piece of epidermis, the whole matter is explained without any difficulty. It is entirely for you. We are dealing with a mark which is some 6 in. long—I do not know exactly, but that is near enough, and in which it is said by the witnesses for the Crown they can see the marks of the knife. If so it is a very remarkable coincidence. In order to satisfy you that it is not Cora Crippen the defence must have satisfied you that there is no scar there, because then, of course, if there was a scar there, coupled with the pyjama and the camisole and the combinations and the vests, you have to ask yourselves have you any doubt that that is the body of Cora Crippen.

Now, I must for one moment consider the theory of Dr. Turnbull and Dr. Wall. I wish to speak with great respect, and it is not for me and it is not for you to judge. You saw Dr. Wall in the witness-box. You saw him roll that piece of flesh over twice. They account for the left side of the horse-shoe, as Mr. Tobin called it, by saying it is caused by a fold; and in order to make it the shape, broader at the bottom and narrower at the top, you have to fold it twice over; and you saw yesterday that with the piece of flesh which then would be freshly cut from the body, it is possible either to extend it or to fold it twice over.

You must ask yourselves why anybody who was burying the body should roll it twice over; one cannot understand why; it is entirely for you. If you think it possible, of course you may attach importance to the theory of the rolling over. But observe what you have to deal with. You have got a mark which resembles a scar, sworn by the witnesses for the Crown to the best of their belief to be a scar similar to scars they have frequently seen, sworn by the witnesses for the defence to be not a scar at all, but to be caused by a fold. Gentlemen, it is entirely for you.

As for the last part of the evidence so far as it depends upon medical testimony. What was the cause of death; who put that body there; was it the same person who killed her or not? Well, these hypotheses only need to be stated to be answered. You will not think, probably, that there was more than one person mixed up in this, and probably you will be of opinion that examining the question of who killed her answers the question who buried her. A great deal has been said, not unnaturally, by Mr. Tobin, as to the skill of the man who did it. It has been sworn by the witnesses for the prosecution that a man who had studied in anatomical schools and had seen dissection, and had a knowledge of anatomy, would have quite sufficient knowledge to do this. The answer by Crippen was, not that he has not sufficient knowledge, but that he has not sufficient practical experience, and has never done a *post-mortem* examination in his life. Again, it is entirely for you, but, as I have said, in all probability you will be of opinion that

the person who caused the death of Cora Crippen took steps to get rid of the body. That is the natural thing that a man would do who had committed a great crime.

Now, was she poisoned or was she not? That is entirely for you. You have got evidence that every organ was healthy, and you have got evidence in the early part of the case that on the night of January 31st, and for weeks before, the woman was in perfect health and spirits; no suggestion of illness or ailment, and no organ shown to be affected. Can you account for death by any natural cause? And again, although Mr. Tobin has cross-examined with great ability as to whether the Crown were right in specializing hyoscine, he has not suggested any other cause of death than poisoning. It is possible, of course, that the woman may have been stabbed, or shot, or something else, but the remarkable thing is that there is no wound on the part of the body that is there, except what is necessary to separate it from surrounding things; showing to a certain extent some medical knowledge, if not very high medical knowledge. The doctors said that there was no wound on any one of the organs they examined; there was the heart, liver, and the kidneys, and one or two things I need not mention, and there was no wound except what was necessary to separate it and take it out.

Now, the Crown start their examination. Here I simply endorse what was fairly put to you by Mr. Muir. When people are conducting an examination by analysis, and the result of which could only be established by analysis, I do not think it is right to suggest that it is "unfortunate" that they knew that the man had bought hyoscine. They were looking for several things. Mr. Tobin did say that he did not mean to pass any imputation, and we ought to make great allowance for Mr. Tobin; but I do not think, if you are satisfied with Dr. Willcox's evidence, that you ought to come to the conclusion that he was influenced one single bit by what he knew; he has sworn he was not.

And what did he tell you? He said, "I examined the remains; I took parts of the liver, the kidney, and so on; I went through a process which lasted a fortnight or three weeks; I found something I was able to distinguish as a vegetable alkaloid. As far as my medical knowledge goes, and as far as the medical knowledge of any book produced up to the present time goes, Vitali's test, which is to find out whether an alkaloid is a vegetable or an animal alkaloid, is only operative in the three vegetable alkaloids—atropine, hyoscyamine, and hyoscine; none of the others give a purple colour." Dr. Willcox says, "I did that; I did not know what I was going to find, but I looked to find a vegetable alkaloid, and I found it must be a vegetable alkaloid."

And remember, upon the question whether it might be an animal alkaloid, the result of putrefaction, the only man who has ever got an animal alkaloid at all from any body, meat or human flesh, is Dr. Luff. Therefore, so far as evidence goes, both of literature and fact, there is no witness who has pretended that an animal alkaloid has been found to respond to Vitali's test. Then, said Dr. Willcox, "I had to distinguish between atropine, hyoscyamine, and hyoscine. I applied the bromine test; I should have applied the heat test if I could, but you do not get in a human body that has been poisoned enough to apply the heat test; I could not use it."

It is not as if he said he did not know of the test or did not think it a good test, but he could not use it. "I used the bromide test, and under that crystals would have come out if it was atropine or hyoscyamine; gummy or resinous matter would be produced if it was hyoscine; I found the gummy matter. I tried it again on pure matter, with the same result." He also said, before he found them vegetable alkaloids, he found that all four of the solutions were mydriatic, that is to say, they affected the eye by paralysing the pupil, and that was not disputed as a test by Mr. Tobin, Dr. Willcox further said, "I took genuine specimens and applied these tests, and they exactly corresponded with what I found"; and Dr. Luff said, "I went through the same operations; Dr. Willcox's tests are the best that can be applied, and they produced the conclusions that he has vouched."

Not one single witness on behalf of the defence has ventured to say that that is wrong. Mr. Wynter Blyth, who came here, said that he quite agreed that Vitali's test identified the vegetable alkaloids as far as present knowledge was concerned, but he said he thought—either on the faith of some Italian book or because he had changed his opinion—that it would be found that there would be some animal alkaloids which might give the same results. Gentlemen, it is entirely for you. You have got to act upon the evidence; you must ask yourselves, Have you any doubt that the evidence of Dr. Willcox and Dr. Luff is right? That of Dr. Willcox is most important upon this—that he found hyoscine in the body, in the stomach, kidney, intestines, and liver. He calculated that in the body there was as much as half a grain, and that is not denied to be a dangerous—in fact, a poisonous—dose, a dose that never ought to be given to any human being. If you come to the conclusion that hyoscine to that extent, or anything like it, was found in the body, you will have to ask yourselves have you any doubt that that woman, by whoever it was done, was poisoned by hyoscine? If the cross-examination of Dr. Willcox leads you to the conclusion that he was mistaken, or anything of that kind, then, of course, you may reject his evidence; but, for your own satisfaction, if you are going to do something of that kind, you probably ought to have some theory of your own as to what else caused the death. The one test as to whether it was a vegetable alkaloid is said by Dr. Wynter Blyth to have been the right test, and, though he says he has changed his opinion, he has never himself found an animal mydriatic alkaloid that acted in the same way under Vitali's test. Given that it is one of the three vegetable alkaloids, I suppose it makes some difference, though not much; we have not been told how much more or less poisonous atropine is than hyoscine; it does not matter, because nobody denies that if you cannot get enough for the melting-point test, the bromide test is the right one. You must make up your minds whether there was hyoscine in the body, and whether death was caused by it. What is the effect of hyoscine poison? how can it be administered? It has a bitter taste, but can be readily given in beer, stout, or coffee, or anything that disguises the taste. It produces pain at first, delirium, paralysing of the pupils, drowsiness, coma, and unconsciousness, and death in anything from one to twelve hours. Therefore, from the point of view we are considering, what happened in that house? Some one gave Belle Elmore hyoscine, and she became unconscious, comatose, and died, and there was the dead woman in the house. Where did that hyoscine come from? It may be a coincidence, gentlemen. Are you satisfied with the account the defendant has given of what he had to do with the hyoscine bought on January 19th—never bought before in England by him, never bought since; two-thirds of it used, made up into hundreds, if not thousands, of little tabloids, put into bottles, kept in a cabinet in his room, and the remainder of the hyoscine left in his room when he went away. Where are those bottles? You must ask yourselves, are you satisfied with that account? He has given his account; it is for you to say whether you believe it.

Medico-Ethical.

The advice given in this column for the assistance of members is based on medico-ethical principles generally recognized by the profession, but must not be taken as representing direct findings of the Central Ethical Committee, except when so stated.

PRESS NOTICES OF MEDICAL PRACTITIONERS.

A CORRESPONDENT (Maxim) sends us a cutting from a local paper announcing that a recently-vacated practice has been taken over by a fully-qualified practitioner, whose name and qualifications are given and some details about his past public appointments; we are asked to express our opinion upon it, and to say whether it is serious enough to be reported to the local Branch of the British Medical Association.

*** As we have repeatedly pointed out, it is unfair to suppose that medical practitioners are responsible for everything about them that appears in local newspapers, and in this case our correspondent himself suggests a natural means by which the reporter could have obtained the information without the practitioner concerned being a party to it. If our correspondent possesses any direct evidence that the paragraph was inserted as an advertisement the matter is different, but in the circumstances we think the practitioner concerned should be given the benefit of the doubt, and we may add that his call should be returned.

BRANCH SURGERIES.

P. H.—As often pointed out, it is not in accordance with professional etiquette for a medical practitioner to put up a plate on premises where he has no bona-fide tenancy, but there can be no objection to a branch surgery in a district where the population is so scattered as to have no medical practitioner within five miles.

THE TITLE OF "DOCTOR."

CAPTAIN O. C.—The question of the right of medical practitioners to call themselves "Dr." has been discussed *ad nauseam*. We do not think any of the Royal Colleges has penalized or would penalize a diplomate who used the title of "Dr."

THE General Purposes Committee of the Middlesex County Council has selected Mr. G. A. Cohen, Mr. Ernest Ebblewhite, and Mr. A. M. M. Forbes out of seventeen applicants for the office of coroner of the central district. Mr. George Alexander Cohen is a Bachelor of Medicine of the University of Edinburgh, and a member of the Middle Temple; until recently he was a member of the Middlesex County Council. Mr. Ebblewhite is a barrister and ex-mayor of Hornsey.

Obituary.

SYDNEY RINGER, M.D., F.R.C.P., F.R.S.,

LATE PHYSICIAN TO UNIVERSITY COLLEGE HOSPITAL AND HOLME PROFESSOR OF CLINICAL MEDICINE, UNIVERSITY COLLEGE, LONDON.

ON Friday, October 14th, at his house at Lastingham in Yorkshire, died Sydney Ringer, at the age of 75. With his death a once-familiar figure passes from amongst us.

He had been ailing for some two years and had changed not a little, though to the casual acquaintance this might not be very apparent. A few weeks ago he suffered a slight cerebral seizure, from which, however, he seemed to be making an excellent recovery, so much so that it was anticipated that before long he would be able to be moved south, to the house and care of his married daughter, Mrs. Charles Kayler, at Mixbury in Northamptonshire. This move, which was much in his thoughts and wishes, was not to be. On October 13th a fresh seizure occurred, he became unconscious, and sank rapidly, passing away in the early hours of October 14th, happily not before he had been joined by his daughter and son-in-law, who, travelling up at a moment's notice, reached Lastingham just in time. A few days before his death Dr. Rose Bradford, his friend and former pupil, had been summoned to his bedside, much to the patient's pleasure and comfort of mind, and a short time previously, before the occurrence of the first stroke, another old pupil and friend, Dr. Batty Shaw, had visited him. These reminders that he was not forgotten, in particular by those with whom he had worked, were very pleasing to him. Throughout his illness Dr. Tetley of Kirkby Moorside was most kind and painstaking in his attendance.

Sydney Ringer was born at Norwich in 1835. He was one of three sons of John M. and Harriet Ringer; the eldest son was named John Melancthon, then came Sydney, then Frederick. The father died whilst the boys were quite young, leaving the family in very poor circumstances, but bequeathing to them a much-respected name. 'All three boys belonged to the honourable company of those who carve their own fortunes. John Melancthon —the name is suggestive of a stalwart Nonconformity, which was, in fact, their parents'—went out to Shanghai, and there amassed a large fortune; Frederick Ringer went to Japan, and became so successful and so well known that he was called the "King of Nagasaki," and in the commercial world it was said that his name was as good as a passport from Cairo eastwards to San Francisco and even beyond, right into America; Sydney Ringer was scarcely, if at all, less widely known, for his book on therapeutics carried his name throughout the British Empire and its dependencies, whilst many translations made it familiar in other countries. All three as boys and men were devoted to their mother (the father they could scarcely remember), though to the outside world she seems to have shown an austerity which to the young in particular was rather awe-inspiring. Another old friend and one-time pupil of Sydney Ringer recalls this, together with the fine strong features and strong character of Harriet Ringer; and he is of opinion that the son inherited many of his qualities from the mother. The same informant thinks that Sydney was educated at a school at Norwich kept by a Mr. Brooke, and this may have been so; but another record says that for a time, at any rate, he was under Dr. Brewer, author of the well-known *Guide to Science*. The report goes on to say that this gentleman, unaware of the inquiring mind upon the school premises, made the rash offer of a penny for each most searching scientific question which the boys could produce, and that young Sydney excogitated so many that the offer had to be withdrawn. Whether this was the beginning of the boy's scientific inquiry the present writer cannot say, but it is certain that the stream of questions never ceased until the very end, to the embarrassment of not a few besides Dr. Brewer who came across this eager, agile mind.

By the timely help of a near relative, Mr. Samuel Browne, a medical career was made possible, and after a short period of pupilage at Norwich Ringer came up to London and entered himself at the Medical Faculty of University College in the session of 1854–5. Thence onwards his career was one of uninterrupted progress from post to post. In 1860 he graduated M.B. at the University of London, and in 1861-2 held the appointment of Resident Medical Officer, adding greatly to his growing reputation by the vigour and capacity which he showed during his tenure of this office. In 1863 he graduated M.D., and in the same year was appointed Assistant Physician to the Hospital. In 1866 he became full Physician. Jenner, Wilson Fox, Russell Reynolds, were all members of the staff at this period; Walshe had just left. Of these it was Jenner, another distinguished East Anglian, who gave him his chief lead; indeed, it was upon the example of that great clinician that he modelled his own clinical teaching. In the earlier years of Ringer's work at the hospital it was in the out-patient room that he excited the admiration and enthusiasm of the students by the impressive way in which he taught, and it was a consciousness of this perhaps which caused him to relinquish that department almost with regret when the time came for promotion to the wards. The students, however, had no cause to regret the promotion, for the teaching went on with unexampled enthusiasm and success during the many years which succeeded, until in the year 1900 he retired from active service at the hospital on his appointment as Consulting Physician.

Ringer took much interest in children's diseases, and during the years 1864–69 he was attached to the Children's Hospital at Great Ormond Street as Assistant Physician. This experience of pluralism in appointments does not seem to have impressed him favourably, for the writer recalls a dictum of his that "a man is a fool who holds two hospital appointments"—a pronouncement more forcible than consoling to the medical modernist.

In addition to his work at the hospital, Ringer held for many years the Professorship of Materia Medica and Therapeutics, but it must be confessed that in his lectures materia medica, as such, received scant attention, and that the teaching resolved itself into therapeutics pure and simple. His book was a striking illustration of this attitude of mind; it took its origin in a request from the editors or publishers of Pereira's *Elements of Materia Medica and Therapeutics* that he should undertake a new edition of this monumental work. Ringer's *Therapeutics* was presented in response, and declined, on the grounds that by no conceivable stretch of the imagination could the book stand as an edition of Pereira. The fact was that Ringer was far too eager to get to hand grips with the powers contained within the medicaments to have patience to spend time in describing their outside appearances; in other words, his bent was essentially towards pharmacology rather than towards descriptive and historical materia medica From conversations with Dr. Ringer the writer seems to recall that it was Dr. Parkes, of hygienic fame, who first suggested pharmacological investigation to him as a fruitful field. Coming from such a source, the suggestion would carry great authority, for Ringer had a particular admiration for Parkes, but the sequel showed clearly that the soil was more than congenial, and that here lay natural affinities. Accordingly the step from the lecture room and the wards to the laboratory was quite in the course of things. Upon the resignation of the Chair of Medicine by Russell Reynolds the lectures devolved upon Ringer, who, of course, relinquished at the same time the therapeutic professorship. The lectures on medicine were delivered for some years, but it is more than doubtful whether these were given *con amore;* certainly the genius of the man did not shine so manifestly here as at the bedside, though, for all that, the course was an excellent one, and thoroughly practical. During this period the work in the pharmacological laboratory went on unremittingly, and all those who shared his labours there will agree that these were an unalloyed joy to him, and absorbed the whole man, even to serious conflict with the demands of clinical work. Here lay an unquestionable danger, but somehow it was overcome, and he managed to satisfy the claims of an extensive practice, in addition to those of his hospital duties, and yet found time for his beloved pharmacology. How this was compassed is hard to understand, but the explanation lay partly in his exceptional power of rapid thinking, partly in his mode of life, which was planned for and wholly devoted to his work. Thus, he would rise early, dispatch a hasty breakfast at eight, and the next few minutes would see him on his way to hospital, always on foot, carrying, perhaps, with him some casual co-breakfaster, astonished at the celerity of things. The hospital visit would generally, in

pharmacological days, conclude with a quick-change appearance in the physiological laboratory—Ringer the physician transformed into Ringer the pharmacologist. Upon the patiently plodding laboratory assistant these visits came not unlike electric shocks, from which he would scarcely have recovered before he would find that it was all over: "Fielder!" would have been hailed, a tracing taken, various suggestions made, and off he was again on his way back to Cavendish Place and the morning's consulting work—gradually, very gradually the assistant's breath would revert to fifteen per minute. The afternoon would next be filled with visits, consultations, the hospital round, a *post-mortem* examination, and again, when possible, and for as long as possible, a visit to the laboratory. His attraction to this workshop was quite wonderful. Nothing could withstand it, not even the college palings, over which he clambered in the dark, upon one occasion at least, when the gates would not yield.

A great feature in Ringer's daily life was punctuality; somewhere this has been called *la politesse des rois*. Certainly it is a great virtue, when found, in kings or elsewhere. Ringer possessed it, and whilst economizing his own time respected that of others. The punctual dinner following upon the busy afternoon was itself followed by the post-prandial chat, and then the thinking would go on afresh, either in the armchair or at the table, the materials of a fresh paper spread out before him. At last came the early couch, Ringer showing great wisdom in allowing himself an ample rest; by ten, or soon after, he was generally in bed.

The best obituary notices, the most instructive, are those which the Emperor Marcus Aurelius has given us: "From Rusticus" I learnt this, "from Alexander the Grammarian" that. What, we may ask, did we generations of University College students learn from Ringer? First and foremost to be open-eyed and open-minded; then to be honest. He never confirmed his diagnosis by an avoidance of the *post-mortem* room; on the contrary, he there sought confirmation of its correctness or conviction of its error. He might be hasty, might seem to us to jump to conclusions; this was temperamental. But he never juggled with facts, never consciously strove to make things fit; facts were precious things to him. Further, he taught us by example to be strenuous; his life was full to the brim with energy, which he did not allow to remain potential, but forthwith made dynamic. Those who knew him more intimately—his house-physicians and assistants—saw a great simplicity in his life; he hated display of all kinds and all affectations. In a luxurious age he kept to simple ways, and ruled his life upon the old maxim of "early to bed and early to rise," which maxim did not leave much room for dinings-out, *conversaziones*, theatres, and all the rout of distractions. He saw little of these things in the middle period of his life, nor did he, so far as the writer knows, belong to a single club. To come in contact with Ringer was very tonic, and his daily morning visit to hospital had much of the effect of the cold tub—a little startling but very exhilarating.

Had he chosen to concentrate all his energies upon clinical work, there is little doubt that he would have achieved as great a position as Jenner—he was the same type of man, held the same possibilities. On the other hand, if he had gone through the mill of scientific training from the very beginning, and had spent himself upon science exclusively, he must have gone very far indeed—how far it is only possible to guess from the lengths which he did actually accomplish. Physiologists are realizing more and more the originality and far-reaching nature of his work on the antagonisms and balance of the chemical ingredients of the fluids and solids of the tissues; and amongst a multitude of other investigations, his work on lime salts, their effect upon muscular contraction, upon the act of clotting in blood and of curdling in milk, have thrown a great light upon the chemistry of the tissues and the maximal effects of minimal quantities. But it is not possible to enter here upon an examination of his pharmacological work; it is too extensive; this must fall to others to appraise at its just value. It is, however, interesting to contrast with this work the clinical side of the man, as shown, for instance, in his book on therapeutics. This book went through thirteen editions, and its success was entirely justified, because, in spite of many and obvious defects, it embodied as a fundamental principle the *symptomatic indications and contraindications for the use of and avoidance of the medicaments*. This is what we are all in search of, as practical physicians, and this is the *aim* which he set before us on every page.

Looked at from every point of view of his work, Ringer was undoubtedly a big personality.

What else remains to be said? Much, very much, which must remain unsaid; in the first place because this brief sketch pretends to no more than an impression, and next because Ringer kept so much within, which he did not show to others. Still there are some gaps which may be filled in: he had a strong aesthetic side in spite of the little time which he gave to its cultivation. Notably mention must be made of his great love of music, which a little encouragement might well have developed into a passion; his figure was a familiar one at the Queen's Hall concerts on Sunday afternoons. Ringer disclaimed a knowledge of poetry, yet he had a singularly just appreciation of some of its finest outpourings—for instance, Wordsworth's "Happy Warrior." In like manner he pretended to know little about art, yet he saw and admired proportion above all things, and in the later years of his life he was given to frequent the picture galleries; nay, more, he would, somewhat inconsistently (on his own confession) criticize very daringly—to the writer it appeared that these criticisms showed in many cases a surprising insight. The truth is that no one can come into close contact with Nature in any one of her departments without learning many of the secrets of other departments.

Sydney Ringer was brought up in a severe, almost puritanical, atmosphere of Nonconformity, which one old friend and colleague says he loved to look back upon. This atmosphere clung about him to the end; true, during his married life he was drawn in a direction which seemed, superficially, to be almost exactly opposite—namely, that of High Church, but he never mistook the husk for the kernel, and he did not change really so much as he seemed to do. In later years he reverted somewhat, and the spiritual side of Quakerism drew him a good deal. In any changes he was absolutely honest to himself. The kingdom of heaven has to be taken by violence, it is true, but not by violence to one's own conscience. This Ringer never did, nor would he have done so even to get the kingdom of heaven, had this been possible; he was too essentially honest.

Lastly, it must be added that he was very kind, and in his kindness always most practical.

No mention of Ringer would be complete which omitted the name of Ann Ringer, his wife—Ann Darley was her maiden name; she came of an ancient Yorkshire family. Hers was a very noble character—affectionate, sympathetic, unselfish; her influence upon him was great, and in all crises of his life he turned to her with complete confidence. She was the staunchest of friends. Their home life, both at Cavendish Place and at Lastingham, was absolutely united; its very sufficiency, indeed, seemed to be in great part responsible for the retired life which they both led, and which, after her death, became for him more and more the life of a recluse.

On October 14th Sydney Ringer, the questioner, put the last great question; on the 18th he was buried at Lastingham, on the edge of the moors. He lies in the same grave with his wife and by the side of an elder daughter, Annie Ringer, who died young, and in whose memory the beautiful church of Lastingham was restored "by her father and mother."

A pupil of Dr. Ringer writes:

Ringer, at the outset of his career, devoted himself to the study of clinical medicine, and became undoubtedly one of the most acute clinicians of his time, and he maintained this interest throughout his life. All who came under his influence as a hospital physician had many opportunities of seeing the skill and keenness with which he unravelled the intricacies of a difficult case. It is important to recognize this, as those to whom only his name was known associated him almost entirely with other work, therapeutical and physiological in its bearing; but notwithstanding the very large amount of extremely valuable work that he carried out in the laboratory, he never lost his interest in clinical medicine, especially from the standpoint

of diagnosis, and both as an acute and shrewd observer, and as an interpreter of symptoms, he was pre-eminent. When his position as a physician was thoroughly established, his interest in physiological problems was awakened, and for a period of some thirty years he devoted every available moment spared from a busy professional life to continuous and unremitting work in the physiological laboratory of University College. All the time not actually occupied by lectures, hospital visits, and the calls of private practice, was devoted to experimental investigations, and the results of many of these inquiries may be said without exaggeration to have become part of the common stock of physiological knowledge. Some of his experimental work was purely pharmacological in its scope, and he spent much time in studying the action of various drugs and poisons on the functions of different organs and tissues, as for example, digitalis on the heart and arsenic on the skin. Even with the pharmacological work he did not confine himself to the narrower side of the subject, but was attracted immediately by complex problems such as those of the antagonisms of drugs and poisons, and he was fond of expatiating on the different kinds of antagonism and the significance to be attached to them. It was interesting to observe the different attitude of mind with which he attacked clinical problems on the one hand, and those of the laboratory on the other. In the case of the former he was extremely acute and shrewd in observing, and cautious in drawing conclusions, rarely venturing on that pitfall of the physician—explanation—whereas in the case of experimental facts observed in the laboratory he was fond of advancing theories, to be tested rigorously by subsequent work. His acute mind realized how dangerous theory was in the ward, where it is difficult to test its conclusions, and how useful in the laboratory, where it could be tested and rejected or not by the light of further experiment. Thus he was, perhaps, unduly sceptical of many new clinical observations as compared with the readiness with which experimental facts were accepted. Nevertheless the reason at the back of this difference was a sound one and the old one of the fallacy of observation unchecked by experiment. He was quick to see analogies and fond of drawing general conclusions; thus, no sooner did he discover the action of calcium in prolonging the systole of the heart than he extended his inquiries to all the members of the group calcium, barium, strontium, and further instituted comparisons between the action of these inorganic bases and the action of the members of the digitalis group. Although he did much valuable work in pharmacology and was profoundly interested in this science and anxious to promote its progress, yet the work which will always be associated with his name is that which resulted in the elaboration of what is now known as "Ringer's solution"—that is to say, the great discovery of the important part played in vital processes by the inorganic constituents of the body. Here, again, the wide view he took is shown by the variety and extent of the experimental researches he carried out.

Although the bulk of the work demonstrating the important part played by calcium was done on the heart, and especially the heart of the frog, yet the results were confirmed and extended by similar work on other vital processes, such as the contraction of voluntary muscle, the growth of larvae—for example, the tadpole; the activity of living organisms—for example, tubifex; the coagulation of the blood, etc. He endeavoured to test the action of these inorganic salts on all the processes of living tissues. In this work also the problem of antagonism attracted his attention, and many very striking experiments were devised to demonstrate the different effects produced by calcium and potassium on the frog heart and also on the coagulation of the blood.

Although most, if not all, of this work was done before the modern conceptions of the nature of solutions had been developed, Ringer saw early in his work that, in the action of salts, what was then called the base acted more or less independently of the acid, and in some respects his views, somewhat inchoate as they were, anticipated the more modern theory of solutions, ions, etc. There can be no doubt that all this work was of the very highest value, and it is interesting to note again that it was all done in the intervals of scanty leisure in the course of a very busy professional life.

Ringer was extremely generous and kindly in assisting the original work of younger men, and was always ready to afford any real worker any assistance in his power in order to further any inquiry either in clinical medicine or in physiology or pharmacology, and no one took more genuine interest or a keener pleasure in any new fact observed by his pupils or his friends, as well befitted a man who was so excellent a type of a truly scientific physician.

The following letter, which we have permission to publish, has a pathetic interest, as it was written so near the end; it also shows the graceful courtesy of the man. Its occasion was a reference to Ringer's work made in an address on the Use and Abuse of the Lime Salts in Health and Disease, delivered at the opening of the Post graduate Course at the Glasgow Royal Infirmary on September 1st, 1910, by Sir James Barr, and published in the BRITISH MEDICAL JOURNAL of September 24th, 1910 (p. 829).

Lastingham,
Sinnington, R.S.O.,
September 26th.

My dear Sir James Barr,

Thank you, thank you very greatly, for your kind reference to my work in your address. You can hardly imagine the pleasure it gives me. When you have reached my age and have ceased from all work and you receive such a complimentary notice of your work from so great an authority, you will know the pleasure you have given me.

Yours sincerely,
(Signed) SYDNEY RINGER.

MRS. KAYLER regrets that she is unable to reply personally to the many kind letters of sympathy she has received from the friends and colleagues of her deceased father, Dr. Sydney Ringer. She trusts they will accept this acknowledgement of her sincere thanks.

The Services.

TERRITORIAL FORCE.

SCHOOL OF INSTRUCTION R.A.M.C. SECOND LONDON DIVISION.

THE following classes have been arranged in connexion with this school:

Officers' Class.

Preparatory Course for Examination "A."—LONDON: At the R.A.M. College, Millbank (Lecture Theatre), 4 to 6 p.m. Mondays and Wednesdays, beginning on Monday, October 31st. WOOLWICH: At Head Quarters, Woolwich (Brookhill Road), 3 to 5 p.m. Tuesdays and Thursdays, beginning on Thursday, October 27th.

A preparatory course for Examination "B" will be arranged early in 1911.

A course of instruction will begin on November 8th and will continue until about December 22nd, 1910. It will be held at Greenwich—the drills at the Royal Naval College, the lectures at Vansittart House, Greenwich Road.

Classes will be held in accordance with the following syllabus:

I. *Recruit's Course.*—For men on joining who have received little or no previous instruction.

II. *Junior Course.*—A complete course of elementary drill and first aid for men of under two years' service. Men of over two years' service who require a "refresher" course may attend.

III. *Nursing.*—This course is for members of the nursing section, and is intended as a preparation for the Hospital Courses mentioned in Regulations for the Territorial Force, Appendix 12, 6 (III).

IV. *Clerical Duties.*—Elementary duties of an R.A.M.C. clerk as referred to in Territorial Force Regulations, Appendix 12, 6 (VIII).

V. *Promotion Class.*—Special coaching as drill instructors and in the subjects required for promotion by Standing Orders R.A.M.C., paragraphs 280 et seq.

VI. *Transport.*—Theoretical and practical instruction.

VII. *Sanitation.*—A complete course for all sections and ranks. Privates of the general duty section who attend this course and pass the examination are eligible for the fourth rate of corps pay under Regulations for the Territorial Force, Appendix 12, 6 (IX).

Nominal rolls of those intending to join should be forwarded as early as possible from each unit to the adjutant. Against each name should be stated the courses to be taken.

The Organization of Voluntary Aid.

We have recently had several inquiries regarding the War Office scheme for the organization of voluntary aid and the formation of Voluntary Aid Detachments, and more than one correspondent inquires the attitude taken up by the British Medical Association with regard to the instruction and examination of women to qualify for the nursing certificate, whether it must be by qualified medical practitioners or whether trained nurses, may be recognized as teachers or examiners. As far as we are aware, the Association has so far expressed no opinion on the matter, but we understand that the subject is likely to be raised at the next meeting of the Territorial Forces Committee. According to the present regulations, the only bodies that the War Office is prepared to recognize are the County Associations, and it looks to them to see that the instruction is kept up to the required standard; it, however, publishes a provisional list which, it states, may be added to from time to time, of bodies which it approves for the purpose of granting certificates in "first aid." These bodies are as follows: The St. John Ambulance Association, the St. Andrew's Ambulance Association, the National Fire Brigades Union, the National Health Society, the London County Council, and provincial County Councils, where the conditions as regards instruction and examination laid down in the voluntary aid scheme are complied with, the gist of which seems to be that instruction in first aid must be given by a duly qualified medical practitioner and the examination carried out by a qualified practitioner other than the instructor, the instruction in "home nursing" must be given by a duly qualified medical practitioner or a trained nurse, as may be found most convenient, and the examination must be conducted by a medical practitioner or matron of a training school, both of whom must be distinct from the instructors of the class.

Universities and Colleges.

ROYAL COLLEGE OF SURGEONS OF EDINBURGH.

The following candidates, having passed the requisite examinations, have been admitted to the Fellowship:

E. Adam, Whinneyhill; J. C. R. Braine-Hartnell, Cheltenham; J. C. Bridge, Dunfermline; M. B. S. Button, Deal; B. P. Campbell, Edinburgh; D. Cotterill, Edinburgh; O. C. Dorman, Winnipeg; E. C. Dutton, Manchester; W. Fleming, Shantung; J. S. Goodall, London; W. L. Gordon, Edinburgh; R. Hill, Whiteabbey; E. Le R. Hodgins, Lucan, Ontario; I. W. Johnson, Bury; J. W. McIntosh, London; C. MacLaurin, Sydney; S. MacMurray, Belfast; M. M. Mamourian, Ashton-under-Lyne; H. J. More, Edinburgh; C. C. Murison, Captain I.M.S.; J. O'Doherty, Belfast; S. P. Shroff, Edinburgh; A. Simpson, Tranent, East Lothian; V. F. Usher, Nelson, New Zealand; P. L. Watkin-Williams, Bridgwater; R. Wilkins, Longton; H. J. Williams, Plumstead, Cape Colony.

SOCIETY OF APOTHECARIES OF LONDON.

The following candidates have been approved in the subjects indicated:

Surgery.—*†N. S. Adler *†D. E. S. Davies, *E. B. Keen, †J. K. Nariman, †H. S. Tumber.

Medicine.—G. A. Crowe, †E. D. Richardson, †C. J. Thompson.

Forensic Medicine.—J. G. Lewis, G. S. Richardson.

Midwifery.—J. E. S. Sheppard-Jones.

* Section I. † Section II.

The diploma of the society has been granted to Messrs. J. K. Nariman, E. D. Richardson, and H. S. Tumber.

Public Health AND POOR LAW MEDICAL SERVICES.

MEDICAL OFFICER OF HEALTH AND INFECTIOUS DISEASES.

Dr. Charles J. Renshaw (Ashton-on-Mersey) writes: In your reply to "Cymro" in the Journal of October 15th, 1910, would it not be as well to add that the Public Health Act now in force obliges all part-time medical officers of health "on receiving information of the outbreak of any contagious, infectious, or epidemic disease of a dangerous character within the district, he shall visit the spot without delay, and inquire into the causes and circumstances of such outbreak," etc. If this be not added some medical men may misunderstand the reason of the visit or the inquiry.

Mr. John Jas. Johnson, Clerk to the Urban District Council of Leyland, writes: My attention has been called to the issue of the Journal of October 15th, wherein on page 1198, in a reply addressed to "Cymro," it is stated it is "entirely within the discretion of a medical officer of health whether he visits every case of infectious disease notified to him." This is obviously wrong in the face of the regulations as to the medical officer of health issued by the Local Government Board on March 23rd, 1891, which are still in force. I particularly refer you to Clause 6 under the head of duties in those regulations. If you differ from me it will be interesting to know how you come to the conclusion that you have as reported in the paper.

*** We were assisted in coming to the conclusion which appeared on p. 1198 by a communication from the Local Government Board, which will be found on page 183 of Messrs. Knight's Decisions of the Local Government Board for 1903, referring to Art. 18 (6) of the General Order of the Board of March 23rd, 1891, in which communication it is stated that the Board does not construe the order as rendering it essential, as a general rule, that a medical officer of health should visit the place of occurrence of every case of infectious disease coming to his knowledge; yet if his information points to an outbreak of any infectious disease of a dangerous character, the Board regard it as his duty to take the steps indicated in Art. 18 (6). The Board points out that in this respect it is immaterial whether the officer's information was obtained under the Infectious Disease (Notification) Acts or otherwise. The Board has not laid down any general rules as to the interpretation to be placed on the words "outbreak of any infectious disease of a dangerous character," as they are in the habit of holding the medical officer of health responsible for the judgement which he forms on the question whether any particular case or cases of such disease constitute an outbreak for the purpose of the section referred to.

Medical News.

The King has graciously consented to become patron of the West London Hospital, Hammersmith Road, in succession to his late father, King Edward VII.

The Honorary Secretaries of King Edward's Hospital Fund received last week an anonymous gift of £10,000.

Under the will of the late Mrs. Sarah Hancock the Middlesex Hospital and the Great Northern Hospital, Holloway, each receives £1,000.

The students' annual dinner of the National Dental Hospital will be held at the Trocadero Restaurant, Piccadilly, on Friday, November 18th, at 6.30 for 7 p.m., Mr. Henry T. Butlin, D.C.L., P.R.C.S., in the chair.

A general meeting of the Medical Officers of Schools' Association will be held at 11, Chandos Street, Cavendish Square, on Thursday, November 3rd, 1910, at 4.30 p.m., when Mr. R. Denison Pedley will read a paper on the Care of the Teeth in School Children. Dr. Stanley Colyer and others will take part in the discussion.

At the opening of the local hospital of the Rockefeller Institute for Medical Research in New York on October 18th it was announced that Mr. John D. Rockefeller had given £764,000 to the institute, making his total endowment £1,800,000. The hospital has accommodation for seventy patients, who will be selected with a view to the scientific study of their cases.

A debate on the treatment of fractures in the neighbourhood of the joints will be opened by Mr. Robert Jones of Liverpool, before the Surgical Section of the Royal Society of Medicine, 15, Cavendish Square, W., on November 8th, at 5.30 p.m. Fellows and members of the Section desiring to take part in the discussion are requested to send their names to the Senior Honorary Secretary, Mr. Jonathan Hutchinson, 1, Park Crescent, W.

At the opening meeting of the Bristol Medico-Chirurgical Society the new president, Dr. Bertram Rogers, delivered an address on Samuel Johnson and Medical Life in the Eighteenth Century. The gout and asthma mentioned in Boswell's *Life of Johnson* seemed to him the same diseases as those now thus termed, and it was unnecessary to introduce suppositions either of chorea and rheumatism or of angina pectoris, as had been done by two American writers, in order to explain his death. The symptoms recorded justified the ascription of Johnson's death to heart disease, consequent on chronic asthma.

Letters, Notes, and Answers.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* BRITISH MEDICAL JOURNAL *are devoted will be found under their respective headings.*

QUERIES.

D. G. M. M. desires to know of any monograph or book dealing with iron medication and the absorption and elimination of iron by the system.

FOLK LORE would be grateful to members for any information as to books or papers dealing with folk lore in regard to medicine in the West of England.

ASTHMA asks whether "soured milk" treatment has achieved any success in asthma, in which the digestive symptoms were only very slight. He has a patient of 60 who has had bronchitis and emphysema for several years, with occasional attacks of asthma, which has been very little relieved by the usual remedies or by dieting and spending the winter abroad.

*** We have referred this query to a leading authority who writes: There is no reason to believe that the use of soured milk would be of any help in such a case as our correspondent describes.

ARTERIO-SCLEROSIS.

ARTERIO-SCLEROSIS asks as to the best literature on arterio-sclerosis, particularly the involutionary form of disease and its treatment.

*** The following references will probably answer our correspondent's requirements: Articles by Dr. F. W. Mott in Allbutt and Rolleston's *System of Medicine*, 1909, vol. vii, p. 582; by Professor William Osler, in Osler and McCrae's *System of Medicine*, vol. iv, p. 429; by Stengel, *American Medicine*, 1906, vol. ix, p. 195; ibid., 1904, vol. vii, p. 9, and *American Journal of Medical Science*, February, 1908.

ANSWERS.

COUNTRY G.P.—We are informed that carbon dioxide can be obtained from the Carbon Dioxide Company, Lea Bridge Road, Clapton. The treatment is applicable to infants. Our correspondent will find details of the technique in an article by Dr. J. M. H. MacLeod, published in the BRITISH MEDICAL JOURNAL of January 29th, 1910, page 254.

DETECTION OF TUBERCLE BACILLI IN MILK.

DR. GEORGE STEWART (Blackhall, Midlothian) writes: "Tubercle" will find a description of Pappenheim's method of staining in Abel's *Laboratory Handbook of Bacteriology* (Oxford Medical Publications, 1907), page 93. By this method smegma bacilli are stained blue, tubercle red.

LETTERS, NOTES, ETC.

THE DESTRUCTION OF RATS.

WITH reference to Lieutenant-Colonel Andrew Buchanan's paper on Cats as Plague Preventers, which was published in the JOURNAL of August 6th, Mr. Alf. C. Greene, of Kirk Michael, Isle of Man, writes: This isle, as you will probably know, has more almost than its fair share of rats and mice. On first coming here, four and a half years ago, we were told that, if we did not keep a cat or two, we should be overrun with them. My housekeeper had a strange fear of cats, more even than of these pests, so it was decided that traps and poison should be used instead. Later, through seeing a reference somewhere as to the fear rats had of the smell of paraffin oil, and knowing also that all these animals disliked any other smell in their runs but their own, for our protection I decided to test the application of it upon them, and did so with most excellent results. At this time we had mice everywhere, bedrooms included. I caught one and sprinkled it with a little petroleum, and let it go otherwise unharmed, when it immediately made for its run and disappeared, and from that day to two years after we had not one! After this time one or two came again. I caught another, sprinkled it as before and let it go, and since this have been quite free from them. We have had no trouble at all with them for four years now, and I shall do the same whenever the occasion may arise in future. Every one of my neighbours keeps cats; one, whose house is not fifty yards away, keeps no less than half a dozen of them, but they also have plenty of both rats and mice too. We have none of any of the varieties. It seems to me that many of your readers would like to know of this very simple and easy-to-hand remedy, and I hope it may be useful to you to give it them. I also hope you may think that if this were done systematically all over the world it would drive these pests away from their usual feeding places, and by so doing stop them from breeding fast, and that it would be likely to lead to killing them off much quicker than the use of traps, poison, and virus to spread disease among them will ever do. There is no chance of their bodies decaying and being left on any premises, and causing disease thereby, because they fly from all their runs which are tainted with the paraffin smell, and will not come near it.

ST. PAUL'S "THORN IN THE FLESH."

DR. F. D. WELCH (London) writes: In Romans xvi, 22, "I Tertius who wrote this epistle salute you in the Lord," proves that this writer assisted St. Paul, and I am firmly convinced from the wording that St. Paul wrote with his own hand the verses 25, 26, 27. In my last letter I commented on the negative evidence in these three verses and in the salutations of other epistles, and I would remind Dr. George Jones that he has not, as I requested, yet produced evidence *from the New Testament* in support of his statement that these verses in Romans and the salutations of the Epistles I named were written "in large handwriting similar to that used by some myopes." So far as I know he has none; if there is, will he produce it? Also, why has he not replied to my first question?

So far as I can discover, there is no verse in the New Testament similar to the statement in Romans xvi, 22, above mentioned, which says that another writer assisted St. Paul to write Galatians; and as St. Paul states distinctly in Galatians, "I have written unto you with mine own hand," I am firmly convinced that St. Paul wrote the whole of Galatians right through from the very first word to the very last word with his own hand and without help from any other writer. To have done this his eyesight must have been good, and the evidence that has been produced up till now in support of the opinion that his eyesight was defective is found to fail completely when carefully thought out and rightly interpreted.

In support of my opinion that the words "through infirmity of the flesh" (Galatians iv, 13) mean "because all are sinners," might I point out:

1. In the opening verses of this wonderful epistle St. Paul places very prominently before his readers the words, "who gave Himself for our sins that He might deliver us from this present evil world" (i, 4).

2. That chapter iv consists of a most brilliant oration in support of the gospel of Christ which completely sweeps away all opposition before it, and the words used to mean "because all are sinners" are of exceedingly great value in that oration. If used to mean "because I was delayed by bodily disease," they would be of no value whatever.

3. That in the description of St. Paul's preaching in Galatia (Acts xiv) we are told he "fled unto Lystra" (verse 6), "ran in among the people crying out" (verse 14), from which it is quite certain that nothing serious was wrong with either lower limbs, lungs, or larynx. Even after being stoned "the next day he departed with Barnabas to Derbe" (verse 20), and the whole account of this missionary journey suggests that both St. Barnabas and St. Paul were very active in their movements. I can find no statement that St. Paul was delayed by disease of his eyes!

4 Though not connected with the "thorn," I might point out that when referring to bodily disease in another epistle, St. Paul uses the word "sick" and not infirmity of the flesh. "Trophimus have I left at Miletum sick" (2 Timothy iv, 20).

The following are my reasons for thinking St. Paul refers to the visions of another man in 2 Corinthians xii as well as to his own visions, and that this other "man in Christ" is St. Barnabas: (1) "I knew" suggests a past acquaintance. I am well aware some few people translate the word "know" instead of "knew." (2) "Above fourteen years ago" leads us back to A.D. 45 or 46, when, according to Acts xiii, xiv, St. Barnabas and St. Paul were companions on their missionary journey, part of which was in Galatia. (3) "Whether in the body I cannot tell or whether out of the body I cannot tell" suggests that St. Paul did not see these particular visions himself, but that they were told to him. The description was, I take it, somewhat vague in parts, presumably because the "man in Christ" was very greatly amazed at the visions, and could not in consequence describe everything that had happened very accurately. "Of such an one will I glory, yet of myself I will not glory," proves that St. Paul is referring to another man's visions as well as his own. Of his own visions, all St. Paul tells us is "the abundance of the revelations." The opinion expressed about the "thorn in the flesh" came suddenly into my head in two or three minutes one Friday morning when in my surgery.

I do not intend writing any more on the thorn or St. Paul's eyesight, as it seems to me that all the New Testament evidence has been now discussed.

SCALE OF CHARGES FOR ADVERTISEMENTS IN THE BRITISH MEDICAL JOURNAL.

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NOTE.—It is against the rules of the Post Office to receive *poste Restante* letters addressed either in initials or numbers.